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ARTICLES

American Political Science Review Vol. 86, No. 4 December 1992

REDEFINING THE POLITICAL SYSTEM OF THE USSR: MASS SUPPORT FOR POLITICAL CHANGE

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Using data from a national public opinion survey carried out in the Soviet Union during November and December 1989, we explore two attitudes relevant to the revolutionary changes there: (1) attitudes toward change and political democracy and (2) attitudes toward a core component of socialist ideology, the locus of responsibility for social well-being (the state or individuals?). These variables are unrelated, with the sample relatively evenly divided among the intersecting cells of a cross tabulation. While social conflict may be mitigated by the small sizes of absolutely opposing groups, consensus may also be hard to reach. Ethnicity, education, income, age, party membership, and life satisfaction have important effects on these attitudes. We discuss how attitude patterns in our data may be related to the disintegration of the Soviet Union and to problems faced by the independent successor states as they develop new institutions and foster new values.

In December 1991, the Soviet Union ceased to exist. A loose association, the Commonwealth of Independent States, represented the efforts of leaders of 11 newly independent states to retain a measure of continuity and to cushion the shock, both economic and political, of the sudden severing of ties. To many observers, the viability of the Commonwealth, without a decision-making body or regulatory and enforcement powers, appeared highly problematic even at its creation.

We report data on public attitudes toward change and political democracy, collected in seven of the former Soviet republics before the breakup of the Union. However, the questions we asked and the responses to them are not only closely related to the collapse of the former Soviet Union but are also very much a part of the post-Soviet future. All the newly independent states will have to address the attitudes of their populations to fledgling institutions and restructured values. All other things being equal, institutions and values that are supported by appropriate public attitudes will have higher prospects of survival. Difficulties in institutional development in areas where public support is low will be magnified. Each of the new states will, in short, have to confront the continuing public attitudes and beliefs with which they are beginning their political lives.

Further, our survey of attitudes toward the end of the life of the Soviet Union illuminates the profound impact of ethnicity on all the aspects of political change we studied. This variable is particularly important in the analysis that follows and underscores and prefigures the divisions that were later ratified. The depth of these divisions suggests not only a precarious outlook for integrative institutions, but also suggests difficulties for the large number of ethnic Russians living outside their home republic.

SUPPORT FOR CHANGE AND REFORM IN THE SOVIET UNION

In the historical experience of the last two centuries, movements toward democratization have often been accompanied by an enlarged role for the state as the protector of individual liberties and economic well-being. In these new democratic systems, the mass public was protected from arbitrary uses of power by institutionalized systems of state control that included both means for political responsibility and accountability to the public and the protection of social welfare; the latter typically involved governmental control over the provision of many social services. Many of the Western European democratic socialist movements stem from this sort of historical experience. In established democratic systems, as well, increasing political democracy has frequently meant an enlarged role for the state in the provision of social welfare; the historical experience of the United States since the New Deal period in increasing economic welfare and civil rights follows this pattern.

At present, there are important movements toward decreased state activity in many democratic nations. While these are frequently cited as part of a worldwide movement toward "privatization" that includes the democratic revolutions in the former Soviet Union and the nations of Eastern Europe, the differences between these political contexts are far more important than the similarities. The historical experience of the Soviet Union and the Soviet-type states of Eastern Europe is different in a fundamentally important way: the increased state control that occurred as the political regimes of these nations were formed was not part of a movement toward increased democracy but, rather, of a system of centralized control and repression.

In the Soviet Union (though the record of Gorbachev's perestroika is mixed), an important component was a *smaller* role for the state. Shortly after his accession to power, Gorbachev took such steps as increasing centralization in the agriculture and food industry, restricting unearned income, and instituting the antialcohol campaign, all of which tended toward a greater role for the state. The course was then reversed, and the drive for pluralism began. "Informal" organizations increasingly entered the political arena, breaking state- and Party-run monopolies. The electoral process became, in 1989, genuinely (even if not fully) democratic. Media outlets were rapidly shorn of the tight constraints of censorship and uniformity, though tensions between editors and politicians increased markedly between late 1990 and the August 1991 putsch.

In the economic sphere, official rhetoric included support at the highest level for a market economy. Gorbachev's chief advisor, Alexander Yakovlev, considered the market as necessary for both socialist and capitalist outcomes. The ability to contract directly with foreign businesses was no longer the monopoly of the Moscow ministry; and the ministries were, in any case, in the process of downsizing. The notion of the joint venture, an arrangement to introduce foreign capital and techniques at all levels of the economy, became part of the message of perestroika. If the economy was not revolutionized until after the failure of the August 1991 coup, the policy pronouncements were, nevertheless, much in evidence earlier. Both political and economic reforms (with the political gaining much greater ground than the economic) chipped away at the power of the centralized state and the Party. The attempt of the opponents to regroup and reverse the course of reform may have convinced Gorbachev in 1990, but they ultimately failed in their bolder move the next August.

As noted, Gorbachev's pronouncements, particularly in the early stages of perestroika, were indeed ambiguous; and the tensions surrounding a move to a radically individualist political economy remained unresolved throughout his period of leadership. However, there can be no doubt that a major thrust of reform in the Gorbachev era was, *as compared to what went before*, toward decentralized government, more individual initiative, and greater individual responsibility. These ideas were frequently implied in discussions of "marketization" and greater individual "initiative" and "productivity." These are the ideas that we intended to capture in our survey with our question on state versus individual responsibility.

Because of Soviet history, our measures and our discussion of support for political change take the form of a focus on support for increasing political democracy (which is unexceptional) but also of support for greater responsibility for *individuals* in social welfare as opposed to the continuation of the state's playing the major role in this area. In this respect, our discussion is different from discussions of democratization that assume that this process will involve a larger role for the state. Given the historical and political context, this is a specification that seems to be necessary for theories of democratic development.

Who Supports Political Change and Reform?

Beliefs about the role of the state, group identifications, and personal self-interest are among the most prominent bases of political attitudes. In particular, the understandings held by a nation's citizenry about what role the state should play in the political and economic life of the nation, how far political dissent may be expressed publicly, how important order and stability are when these come into conflict with freedom, and other such basic aspects of the relationship between the public and its political leadership are major parts of the political culture of a nation. Typically, attitudes based on the prevailing political culture tend to remain stable unless cultural upheaval and discontinuity force people to take account of change (Eckstein 1988). Depending on the sort of change that is occurring and the fundamental motives that one believes drive political attitude formation, different hypotheses follow about the social sectors in which one expects support for change to develop most rapidly.

In change oriented from an authoritarian to a democratic form of government or a command to a market economy, attitudes toward change might be driven by beliefs about the role of the state and by personal self-interest. The nature of the change being undertaken is germane to the types of relationships that should be expected, as are the political and social contexts in which change occurs. In general, cultural theories of attitude formation suggest that attitudes supportive of democratization will develop most rapidly in the nontraditional sectors of the nation—that is, among the young and better-educated; in the cities, rather than in the rural areas; in the service, rather than the agricultural, sectors of the economy; and probably among men, as compared to women.

Other theories of attitude formation occasionally suggest contrary—or at least supplemental—hypotheses. For example, to support a market economy in the Soviet Union and its successor states represents a reversal of seventy years of ideological indoctrination. We would not expect people who were ideologically invested in the current system to be very supportive of this type of change, regardless of any "modern" demographic characteristics they might possess; at the least, their support would be tempered by the conflict. Similarly, the market economy and the economic dislocations unleashed by perestroika threaten the self-interest of some groups. Low-income people may be expected to want to retain the support and material benefits provided by the socialist state, regardless of any other "modern" characteristics they may possess, such as residence in large urban areas.

Our analysis takes into account the various theoretical problems and considerations we have discussed. First, we think attitudes toward economic and political change themselves must be clarified because the complexity of change suggests that these may not be unidimensional. Second, to the extent that we can identify a cluster of attitudes that are

TABLE 1

Attitudes on Various Aspects of Change and Reform (%)

QUESTION	REFORM POSITION	"IT DEPENDS"	TRADITIONAL POSITION	TOTAL	WEIGHTED N
Competitive elections (Q29)	95	—	5	100	(1,883)
Income differences (Q22b)	87	—	13	100	(1,910)
Free speech vs. order (Q22d)	58	—	42	100	(1,841)
Individual vs. state responsibility (Q22a)	49	—	51	100	(1,822)
Rapid vs. slow change (Q18)	45	17	38	100	(1,885)
Protest vs. traditional methods (Q22c)	36	—	64	100	(1,826)

Note: Ns are in parentheses. The total weighted N for the sample is 2,006. Missing data ranged from 5% to 9% in these questions. A middle position was available only in Q18.

oriented toward reform, we would expect greater agreement with such values in groups which belong to the "modern" sectors of society. Third, however, we do not believe support for democracy is based only in ideas or ideology; we also take into account how far groups can be expected to benefit from political and economic change. In groups that are part of the more traditional sectors, uniquely disadvantaged by change, or ideologically invested in the present system, we expect to find more resistance to modernizing and democratizing social, political, and economic change. Indeed, it is in such sectors that we expect to observe real resistance to reformist values. Thus, our analysis proceeds from assumptions rooted in both political culture theory and considerations of material and organizational self-interest.

The Political Context of the Survey

Largely unintended and unforeseen, what began in 1985 as perestroika resulted in revolutionary change. Glasnost, most observers concur, despite the tension and conflict over its scope, irreversibly altered the boundaries for the expression of opinions, although in the post-Soviet politics of the successor states (as before), there are wide differences from west to east. The political reforms (in which the monopoly of Communist Party power gave way first to splits in the Party itself and then to the presidential/parliamentary system amalgams in the now independent republics) continue to be implemented with varying degrees of rapidity and commitment. The Gorbachev administration's rhetoric of economic reform, first couched in conservative and ineffectual proposals, approached market principles, then reversed direction before the attempted coup. In some areas, such as the largest cities and the Baltic republics, reform proceeded far more rapidly than in the provincial heartland; and the increasing de facto decentralization of the country's political and economic systems has also been accompanied by severe economic deterioration and interethnic conflict. The main beneficiaries of the older system—the armed forces, the security organs, and the professional bureaucrats (the apparat) of the Communist Party and its remnants—have been least willing to see their power and resources eroded.

It is in the context of rapid and radical change that our survey of Soviet public opinion was designed and conducted. One of our aims was to explore the attitudes of our respondents to the tempo and components of political change. Our analysis is based on a national sample of interviews collected during late November and early December of 1989. These were true personal interviews and not the "drop off, pick up" questionnaires sometimes referred to as "face-to-face interviews" in the Soviet Union; this is the first large-scale true personal interview survey in that nation. The data were collected by the All-Union Center for Public Opinion Research, directed by T. I. Zaslavskaya. Further details on the sample and field procedures are given in Appendix A.

ATTITUDES TOWARD CHANGE: DIVISION AND IDEOLOGICAL COHESION

We focus here on six questions designed to measure attitudes about changes taking place in the Soviet Union at the time of our study. One question (Q18) is about the pace of change itself, and five more specific questions ask about change in the political and economic arenas: the perceived locus of responsibility (individual or collective) for individuals' social well-being (Q22a), the acceptability of differences in individual incomes and standards of living (Q22b), the acceptability of nonconventional forms of political expression like strikes and demonstrations (Q22c), the priority of free speech over public order (Q22d), and support for a competitive electoral system (Q29). The back-translation from Russian for these questions is given as Appendix B. (R) next to a response indicates that it was scored as "reformist" or "change-oriented." Except for Q18, which had a middle position ("In different situations, act differently"), all questions were dichotomies. The distributions on these questions highlight the substantial disagreement that existed in the public on some issues that were important aspects of glasnost and perestroika (see Table 1).

As Table 1 makes clear, depending on which particular question one looks at, one can come to quite different conclusions about the level of support for change that existed prior to the dissolution of the USSR. To begin with, there was substantial disagreement on how fast change should be carried out. In the realm of political expression, practically no one objected to competitive elections. Nevertheless, apart from this very simple—albeit basic—measure of democratization, it was clear that old ideas were dying slowly; more subtle concepts about political rights were not universally approved. Two-fifths of our sample believed that public order was more important than free speech, and only slightly more than a third were willing to accept unconventional forms of political participation as a legitimate part of the political process.[1] Still, while a clear majority preferred to solve problems using conventional means, considering the strength of the wording of the question about protest, the amount of support for strikes, protests, and demonstrations was remarkable.[2] Nevertheless, the ambiguity and conflict over the extent of permissible political conflict signals the fragility of the current order.

Relatively few people were hard-core believers in strict income equality (about an eighth of our sample). Note that strict income equality has really not been a feature of official political doctrine. Stalin and all subsequent leaders asserted the rationale for wage differentials and there was a brief period of loosened reins on entrepreneurial activity during Lenin's New Economic Policy (NEP). However, as David Lane notes, "until the advent of Gorbachev's leadership, differentials were relatively low" (1990, 32); and there has been an official bias against too large a spread of income differences and against unearned income. Indeed, directives relating to the latter, even under Gorbachev, seriously impeded the development of individual economic initiative. It is also possible that the relatively low popularity of income equality in our sample may have been encouraged by the wording of our question, which accorded the *state* the function of providing opportunities for individuals to earn as much as possible. In retrospect, the question probably should have emphasized the exercise of individual initiative for making as much money as possible, rather than state sponsorship of these opportunities.

Other aspects of the economic "old order" are clearly more durable. Most striking, there was virtually an even division in the sample between individual and state responsibility for individual well-being. Since this is a central aspect of what change in the political and economic systems of the former republics is all about, the lack of consensus on this item suggests the potential for serious divisions in the former Soviet states.

TABLE 2

Relationships among Attitudes toward Political Change

QUESTION	Q22A	Q22B	Q22C	Q22D	Q29
Rapid vs. slow change (Q18)	−.19	.18	.29	.25	.55
Individual vs. state responsibility (Q22a)	—	−.03	−.14	−.05	−.13
Income differences (Q22b)	—	—	.60	.55	.41
Protest vs. traditional methods (Q22c)	—	—	—	.76	.60
Free speech vs. order (Q22d)	—	—	—	—	.59
Competitive elections (Q29)	—	—	—	—	—

Note: Values are gammas.

It is also evident that there is not one cohesive ideology that ties together all of these ideas about political and economic reform, although we do find a cluster encompassing five of the six items. Correlations involving the question about state or individual responsibility for social well-being are particularly interesting. Table 2 presents the relationships among all six questions used as dependent variables. Many of these correlations are consistent with our expectations. For example, those who support free speech are also likely to approve of unconventional forms of political participation. However, being a "political democrat" or wanting rapid change does *not* necessarily mean that one is eager to abandon the socialist system by opposing a major role for government in caring for individuals. In fact, the reverse tends to be true. For example, people who approve of protest are *more* likely than those who disapprove to see individual well-being as a state, rather than an individual, responsibility. Moreover, those who want rapid change are *more* likely than those who want slow change to see individual well-being as a state responsibility. Indeed, the question on individual versus state responsibility, considering the "individual" response as the more change- or reform-oriented attitude, is negatively related to each of the other items under consideration. Although these correlations are modest, the data suggest that support for many aspects of change and political democracy is unrelated to the posture toward collective versus individual responsibility for the social welfare of citizens.

At a theoretical level, these unexpected negative relationships demonstrate that the ideology of change must be understood in its specific political context. A 1980s American perspective that equates political democratization with a limited role for central governments (whether all-Union as in the former USSR or in the newly independent states) may be considerably off the mark from another geographic or temporal perspective. Nevertheless, it remains true that even in the specific case of "perestroika," our results are unexpected. Since perestroika was generally understood to include *both* political democracy and a move toward marketization and smaller centralized government, responses to the questions we used might have been expected to be unidimensional. It is clear that they are not. Perestroika was a complex, often contradictory cluster of ideas; and some aspects were clearly more popular than others. Furthermore, while there was strong support for political reform in the Soviet Union, this did not mean that all the ideas to which people had been socialized since the Russian Revolution would be equally changeable in the short term, even when new ideas that may appear inconsistent with some of them are introduced. The Soviet population had been exposed—subjected—to 70 years of official doctrine about the primacy of the state and collective approaches to problems. As Table 1 makes clear, this socialization was far from uniformly effective. But such ideas as people had developed about this subject were clearly not immediately changed into wholesale support for individualism and free enterprise even though there was a common recognition that the system had not worked efficiently and institutions supporting increased political and economic democracy had begun to develop. Clearly, political culture and socialization will inevitably provide a drag on attitudinal change.

That many advocates of change continue to support the socialist view that the state, rather than individuals themselves, is responsible for the well-being of the citizenry, suggests that contrary to much popular belief in the West, there has not been a wholesale abandonment of socialist principles. On the contrary, there may even be somewhat more support for these ideas among those who are the strongest believers in individual free expression and rapid change. What many Soviet citizens pressing for change may want is a more efficient and productive state, rather than a state shorn of social obligations. The Communist Party, as the engine of that obligated state, was rejected, but not the obligation itself; the institution failed, but not the norm. Indeed, the institution's failure may have been due largely to its unwillingness or inability to service the norm; and an important part of Gorbachev's failure as a leader was in his failure to distinguish the two.

The differential support for the various components of perestroika and the negative relationship between locus of responsibility and the other items suggest that a one-item measure of "support for perestroika" (such as appears in some reports of Soviet public opinion) would be quite difficult to interpret meaningfully. Further analysis confirms that Q22a, state versus individual responsibility for social welfare, should not be combined with the other items in a general measure of attitudes toward reform. As the negative relationships presaged, factor analysis suggests that Q22a is orthogonal to the other items; on the factor it dominates, all other items have negative loadings. Therefore, we combine items 18, 22b, 22c, 22d, and 29 into a scale of "support for political change." The alpha-reliability of this measure is .51. In addition, we analyze Q22a separately as a measure of "individual versus collective responsibility." While single items are generally less valid than multiitem indicators, in this case the idea we are measuring—attitudes toward individual versus state responsibility—is quite clear; and the single item has obvious content validity as a measure of that idea. We feel that the content validity and the inherent importance of the idea measured by this particular item warrant its use in our analysis.

For the scale of attitudes toward political change, we scored individuals +1 for each of the five items answered in the "reform," or change, direction, and −1 for each item answered in the "status quo," or traditional, direction. Thus, the scale ranged from −5 to +5. The distribution was heavily skewed toward approval for political reform. Almost 70% of the sample had net positive scores, almost 40% chose the reform-oriented response on three of the five items, and 14% were supportive of reform on all five items. In contrast, fewer than 1% opposed reform on four or five of the items, and fewer than 6% chose the traditional response on three items. Clearly, the battle over reform in the former Soviet Union was not between the reformers and those opposing all change but, rather, among those prepared to accept varying degrees of change.

For cross tabulation, the scale was divided into five categories: −2 through −5, −1 and 0, 1, 2–3, and 4–5. As expected from our individual item results, the relationship between the composite index of attitudes toward political change and the single-item measure of preference for state or individual responsibility for social well-being is negative, although rather weak. The statistics and the cross tabulation of the two variables are presented in Table 3. As we have already discussed, given that reform and restructuring in the Soviet Union were defined to include *both* political democracy and a move toward increased individual initiative, had public attitudes on these issues been similarly formulated, the relationship would have been positive, rather than negative. Instead, the negative correlation indicates that those who are more supportive of political change tend to favor state responsibility more than the opponents of change do and that those favoring individual responsibility tend to be less supportive of political change than those favoring continued state responsibility.

In order to clarify the degree of social and political conflict about these issues, it is instructive to consider the percentages of the total sample falling into each of the cells; and these are the figures shown in the body of Table 3 (the reader can calculate row or column percentages from the information given). Of the total sample, 29% are in the corner cells, representing the greatest degree of disagreement; 4% favor continuation of the socialist ideology of state responsibility *and* wants little or no political change (representing the extreme of those supporting the status quo); an additional 5% also favor little or no political change yet prefers a system favoring individual initiative. On the other hand, 12% favor a great deal of political change but prefer the socialist system of state responsibility, while 8% oppose state responsibility in favor

TABLE 3

Relationship between Support for Political Change and Locus of Responsibility for Social Well-Being (%)

SUPPORT FOR POLITICAL CHANGE	LOCUS OF RESPONSIBILITY FOR SOCIAL WELL-BEING	
	STATE	INDIVIDUALS
−5 to −2	4	5
−1, 0	11	11
1	10	12
2, 3	14	13
4, 5	12	8

Note: Weighted N = 1,785 (100%). Entries are percentages of the total sample in each cell. For the full table (with support for political change not collapsed), Kendall's tau $c = -.10$; gamma $= -.12$; $p \leq .02$.

of individual initiative and also want a great deal of political change. Only the last group really advocates a completely new system, and only the 4% mentioned earlier favor holding on to the status quo. While these totally conflicting groups are small, note that the remaining total cell percentages are relatively uniform, so that any group can substantially increase its size by including the group in the next closest cell. Thus, relatively evenly balanced coalitions of many different political complexions are possible.

As in most complex social processes, our data show that attitude change is uneven. New ideas are differentially accepted, and while a majority appear to favor rapid change, there is also significant conflict over the shape that change should take. Thus, some of the political difficulties and pressures from both right and left that ultimately brought down President Gorbachev—and continue to beset the newer leaders—are clearly illustrated in these data. We would suggest that in a society actively constructing new political and social institutions, with a mobilized public whose attitude structure supports division into relatively equally sized ideological groups, any political or economic institutional developments are liable to be uncertain until attitudes coalesce to a greater extent around both acceptable degrees of change and defining political and economic values.

SOCIAL CLEAVAGES

Up to now, we have discussed divisions in the total population of the former Soviet Union. Now, we move to a discussion of the major social cleavages attributable to a variety of social and demographic factors. Our analysis focuses separately on several major variables because we think it important to understand how particular variables operate to cause social cleavage in the process of change; at the end, we discuss regression equations that combine the most important of these variables.

Ethnicity

As the break-up of the Soviet Union and its reemergence as a number of individual states suggests, ethnicity was among the most important factors in considering support for the kinds of changes initiated by the Gorbachev administration. Our analysis includes the principal ethnicities that fell with sufficient frequency into our sample: Georgians, people from the Baltic states (in our case virtually all are Estonians), Central Asians (principally Uzbeks and Kazakhs),[3] Ukrainians, Belorussians, and Russians.

The differences among the nationalities in support for political change are shown in Table 4. While Russians, Ukrainians, Belorussians, and Balts did not differ very much from each other in the degree of support for political change, Georgians appear to be somewhat more favorable to change than the others, and Central Asians were considerably more resistant to change. In the post-Soviet configuration, the Georgians under Gamsakhurdia became outliers rejecting the notion of even weak Commonwealth ties, while many of the Central Asians have been able to prolong the previous system under new political labels.

The pattern of ethnic responses on the question of state versus individual responsibility for an individual's well-being (see Table 5) reflects the same contradictions that we have just discussed. For example, Georgians very strongly favored the socialist ideology of the state's taking responsibility for the individual; yet, of all ethnic groups, they were the most supportive of political change. The Georgians' response to the question of state versus individual responsibility

TABLE 4

Support for Political Change by Ethnicity (%)

SUPPORT FOR POLITICAL CHANGE	RUSSIANS	UKRAINIANS	BELORUSSIANS	BALTS	GEORGIANS	CENTRAL ASIANS
−5 to −2	7	5	8	3	2	24
−1, 0	20	21	20	19	18	33
1	21	24	23	24	15	20
2, 3	29	34	19	41	32	15
4, 5	23	16	30	13	33	8
Total	100	100	100	100	100	100
Weighted N	(1,119)	(275)	(70)	(42)	(53)	(208)

TABLE 5

Locus of Responsibility for Social Well-Being by Ethnicity (%)

LOCUS OF RESPONSIBILITY	RUSSIANS	UKRAINIANS	BELORUSSIANS	BALTS	GEORGIANS	CENTRAL ASIANS
State	51	54	50	41	77	43
Individuals	49	46	50	59	23	57
Total	100	100	100	100	100	100
Weighted N	(1,043)	(244)	(64)	(39)	(51)	(206)

may well have reflected their attitudes toward an independent Georgian republic, rather than a centralized Soviet state. Ronald Suny (1988), in discussing components of Georgian nationalism, emphasizes the ability of Georgian republic leaders to staff Party and state institutions with Georgians to the exclusion of other nationalities. Well before the Brezhnev policy of *korenizatsia* (nativization), the Georgians had consolidated their hold on the institutions of power and influence in their republic. Suny argues that even though an independent Menshevik state had existed briefly, the most "complete consolidation of the Georgians as a nation came in the first seven decades of Soviet power. . . . [and] the actual evolution of Soviet Georgia resulted in the emergence of a compact and conscious nation" (1988, 318). It is likely that this association of nationalism and the Georgian republic helped to produce the response that people would be better off if one's own (i.e., the Georgian) state provided leadership.

The second anomaly in attitudes toward individual versus state responsibility is exhibited by Central Asians. On the political change scale, they provide by far the strongest support for the status quo, consistent with their more traditional value structure and geographic and psychological distance from the centers of political change. Yet on this question, they differ much less from other ethnic groups (except the Georgians) and display a much greater willingness to place responsibility on the individual than their conservative posture on the political change scale would suggest. It is our view that although Central Asians may be as divided on this variable as the other ethnic groups, an alternative interpretation is more plausible. For some of the most traditional Central Asians the understanding of "individual" in this context may differ from that of other ethnicities. Specifically, it is the case that "individual" for the traditional ethnicities carries with it the sense not of the atomized individual, standing alone, but, rather, the individual "unit," which is, in fact, the traditional family. As Nancy Lubin wrote about Central Asia: "The extended family in pre-Revolutionary Turkestan was both a unit of consumption and production, an independent world unto itself" (1984, 209). Therefore, a response favoring the individual on this question may actually be a response favoring the continuation of the traditional patterns of social organization that have endured in Central Asia (even if in attenuated form on some dimensions) throughout the Soviet period, rather than an indication of support for a change in the current system.

As is well known, the moves for sovereignty, independence, or autonomy on the part of the constituent republics of the former Soviet Union put pressure on the substantial Russian "minorities" (who are often disproportionately found in positions of power and influence) in the non-Russian republics. Some are returning to the Russian Federation; but it is an unlikely solution, considering that at the time of our survey, some 24 million Russians lived outside this republic (Guboglo 1990). This potentially creates situations in which these individuals are exposed to a variety of experiences and peoples that may affect their attitudes in ways not likely for Russians in Russia. Indeed, our data show that the context of the republics strongly affects attitudes toward political change. Thus, we have also explored the impact of republic-of-residence for Russians living outside Russia.

Russians in Central Asia—both Uzbekistan and Kazakhstan—are much less likely to favor political change than are their counterparts in the Russian Federation. Of Russians resident in Russia, 52% score between 2 and 5 on our political change scale. However, only 38% of Russians resident in Kazakhstan and 36% in Uzbekistan were this supportive of change—closer to the 30% of Kazakhs and 23% of Uzbeks (in their native republics) than to their ethnic compatriots, Russians in Russia.

Ukraine is the only other non-Russian republic in which we have a sufficient number of Russians for analysis. Russians living here were *more* supportive of change than those at home: 66% score between 2 and 5 on the scale. Interestingly, while Ukrainians were more supportive of change than the general norm for the sample (51% of those living in Ukraine score from 2 to 5), they were not as supportive as Russians living in their republic. However, our respondents in Ukraine were drawn entirely from the more Russified area of that republic. There were no sampling sites in the western part of Ukraine, where Ukrainian nationalism is most pronounced. Hence, while our sample in Ukraine does not reflect well that much more volatile context among Ukrainians themselves, the attitudes of minority Russians in Ukraine on issues of political change might be less affected by such intrarepublic regional differences. Indeed, in the 1991 vote in the Ukraine on the subject of independence, though there were differences between Russians and Ukrainians, the divisions were far less than had been anticipated.

We find similar results on the influence of the local context for our question on individual versus state responsibility for social well-being. Among Russians in Russia, 50% choose *individual*. However, in Kazakhstan, where 66% of the titular nationality chooses this alternative, Russians choosing it increase to 58%. In Uzbekistan, where 55% of Uzbeks choose *individual*, relatively close to the Russian "norm" for this question, Russians are very slightly greater in their support for the individual, with 52% choosing this response. In Ukraine, where only 45% of the Ukrainians choose this alternative, only 37% of Russians do so.

In Central Asia, the Brezhnev policy of "affirmative action" (promoting people of the indigenous nationality and permitting preferential treatment) resulted in nepotism in which tribal loyalties and command over resources were expanded virtually into fiefdoms (Lubin 1984; Radio Liberty 1982). Russians in these and other republics perceived not only the erosion of their previously privileged status but also reverse discrimination (Karklins 1987). The resistance to change on the part of both the Central Asians and the Russians who live there may arise from values they share, even though the two communities tend to live separately and follow different career paths (Lubin 1984; Olcott 1987). On the other hand, the sources of resistance to change may have been different for the two ethnic communities, arising from the Central Asians' preference for the Brezhnev regime's complacent outlook on local arrangements, both legal and illegal, and, for the Russians, a general unease with what they perceived to be a continuing decline in their status, especially given the high birth-rate differentials and increasing proportions of the titular nationalities (Olcott 1990). Instances of discrimination against Russians in non-Russian republics broke into the press well before the current clamor about language, independence, and citizenship laws (Mikhailova 1989; Novikova, Ovsyannikov, and Rotman 1989). These issues, added to severe economic deprivation and conflict over status and scarce resources, have exacerbated the problem. For Russians in Central Asia, Moscow does not offer much solace: on political ideology, these respondents remained closer to the Uzbeks and Kazakhs than to their fellow Russians in Russia.

That Russians outside Russia often share a general propensity to accept or reject current political ideas in a fashion similar to that of the titular nationality in those republics (and different from Russians in Russia) is also subject to a more general theoretical explanation. The political attitudes of individuals who change their residential locations tend to be influenced by the political and social context into which they move; such individuals tend to change their attitudes in the direction of conformity with their new neighbors. This phenomenon has been found in diverse research, including studies of Americans moving within the United States (Brown 1981, 1988) and Americans moving to other countries (Finifter and Finifter 1989). The addition of this general contextual effect to the real and perceived grievances of Russians in Central Asia may sharpen even further the attitude differences between Russians in the home republic and Russians in the other republics. If, in the aftermath of the development of the Commonwealth of Independent States, pressures for the removal of Russians back to their home republic increase to intolerable levels, assimilation in Russia may be extremely problematic on political grounds, quite apart from the economic difficulties involved.

In the Baltic area, the situation of the Russian minority is different. The Baltic peoples, who took the lead in pressing for political change within the Soviet Union and were the first to declare independence, do not differ significantly from Russians in Estonia on support for political change. Perhaps surprisingly, on the item about strikes and demonstrations (Q22c), the posture of the Estonians hardly differs from the negative stance of the Central Asians in rejecting this form of political protest; and they are considerably more conservative than Russians living in their republic. Within Estonia, 45% of Russians, but only 18% of Estonians, think strikes and protests are acceptable methods of political participation ($p = <.001$, tau b = $-.26$, gamma = $-.57$).

We think this difference in the degree of approval of protest between Russians and Estonians living in Estonia occurs because of the particular socioeconomic location of Russians in that republic. Because the design of economic development in Estonia and Latvia during the Soviet period involved large inmigrations of blue-collar workers, Russians are now concentrated heavily in the industrial labor force of those more industrialized Baltic republics. There were substantial differences in social class between the ethnically Baltic Popular Fronts and Interfront, the Russian response. In Estonia (the Baltic site for our survey), the use of strikes was more likely to be a Russian, oppositionist, protest instrument than an expression of tolerance for political opposition within the ethnic Estonian community. Consequently, it is the Russians, rather than the ethnic Estonians, who account for most of the approval for protest that occurs in this republic.

Age

We expected age to be negatively related to support for reform efforts geared to social and political modernization and democratization. Virtually by definition, youth is part of the "modern" sector of society, and youth groups are frequently among those active in movements for social and political change; conversely, the relationship between aging and resistance to change is a phenomenon widely observed. In the case of the Soviet Union, the propensity of youth to accept change more readily than older people is probably accentuated by the effect of the Stalin period. A "generational effect" has been very widely discussed in terms of elite recruitment and mass attitudes (Bahry 1987; Bialer 1986; Hough 1988). Analysts working with data from the Soviet Interview Project (SIP)[4] have drawn a distinction between the generation whose formative experiences occurred during the Stalin regime and the generation that might be termed *post-Stalin*. The latter—defined as those born in 1941 or later—is clearly deviant from the political conformity of previous generations, even when other relevant variables are held constant (Bahry 1987; Silver 1987). It is argued that this is a "generational" difference and not merely a reflection of the younger age of the post-Stalin generation.

A more recent discussion of the effects of age on propensity to accept change—in particular the reforms introduced by the Gorbachev administration—is that of T. I. Zaslavskaya (1990). In her discussion of the relationship between age and the likelihood of support for reform, she distinguishes two age groups in 1990 most likely to support reform: those over 55 and those under 35. Among the former, she finds those who, as she put it, had a "swallow of freedom" during the reformist Khrushchev years.

In our study, age does have a moderate effect on the support for political change (see Table 6). Since the definition of the post-Stalin generation used by the SIP study encompasses most of our sample—and, indeed, most of the population of the former Soviet Union—we cannot replicate all the generational categories used in the earlier work. However, the strong negative effects of membership in the 60-and-over age cohort do suggest that those who lived for a substantial period of time in the Stalinist Soviet Union are indeed resistant to liberal political reform. All our analyses show that people aged 60 and over are consistently and significantly more negative toward political reform than any other age group. Of those sixty or older, 53%, as compared with only 20% of those under age 60, have zero or negative scores on the scale of support for political change, confirming the political conformity of this group found in the SIP data. By 1991, however, this older group was retiring or being forced into retirement by the Gorbachev program of cuts in state and Party bureaucracies, thus limiting its political impact.

In addition to the marked effect in the oldest age group, our data also show that support for change declines rather consistently with increasing age. As Zaslavskaya observes, for those who were galvanized by the opening to liberalizing reform of the Khrushchev era (though the policies were often contradictory and unevenly implemented), the Brezhnev period represented a closing of that window and the ushering in of an increasingly dreary and immobile system. Thus, among respondents in their fifties or somewhat older, one finds both the initiators of the new Gorbachev reform movement and those who lived under the Stalinist dictatorship. The numbers of the group singled out by Zaslavskaya as favoring political reform, must, therefore, though their influence is very great at present, be offset by the greater part of the cohort. On the other hand, as noted, those who are over 60 are much more opposed to political change.

TABLE 6

Support for Political Change by Age (%)

SUPPORT FOR POLITICAL CHANGE	16–25	25–29	30–39	40–59	60 AND OVER
−5 through −2	4	9	7	8	17
−1, 0	15	19	17	24	36
1	23	14	21	24	22
2, 3	34	29	33	25	18
4, 5	24	29	22	19	7
Total	100	100	100	100	100
Weighted N	(375)	(257)	(385)	(641)	(289)

Note: Tau *b* = −.18; gamma = −.23.

Consistent with these findings, age is also negatively associated with support for individual responsibility for individual well-being, although this effect of age is not as strong as its dampening effect on support for political change (Table 7). However, the relationship is notable for its monotonicity. Clearly, a single cross-sectional survey cannot differentiate between generational and life-cycle effects: the data can be read to suggest either a very regular propensity for people to increase in their support for a collective approach as they age (possibly reflecting a wish to be protected from market forces as individual frailties or other personal difficulties associated with age increase) or a generational effect representing the quite steady erosion over time of the impact of socialization on the collectivist ethos. In regression analyses that we shall report, age is consistently the most or second most important of all variables.

Membership in the Communist Party

From the inception of the Soviet state, the Communist Party was accorded the "vanguard" role, leading and structuring policy options and setting policy direction. For over seven decades, all political discourse in all mass media repeated and reinforced this

TABLE 7

Locus of Responsibility for Social Well-Being by Age (%)

LOCUS OF RESPONSIBILITY	16–25	25–29	30–39	40–59	60 AND OVER
State	43	49	51	53	58
Individuals	57	51	49	47	42
Total	100	100	100	100	100
Weighted N	(353)	(242)	(349)	(594)	(279)

Note: Tau *c* = −.11; gamma = −.14.

notion. Part of the breakdown of the system was the repeal of Article 6 of the Soviet Constitution, which mandated the monopoly of political power by the Communist Party of the Soviet Union; however, this happened relatively late in the process, and our survey was conducted shortly before the event.

If the Party ideology about its vanguard role were valid, it would follow that Party members would differ from the non-Party population with respect to the critical issues of political reform. Given the movement toward glasnost and perestroika, if they were playing a "vanguard" role, Party members would be more supportive of political democratization. We find, instead, that Party membership is not at all related to support for political change. More importantly, of course, this means that Party membership did not predispose to *rejection* of political change. Overall, Party people are as divided about political reform as is the rest of the population. That they do not display a distinctive and coherent position on policies of such import, suggests that at least toward the end of the life of the Soviet Union, the Party was far from the monolithic instrument of Bolshevik tradition.

That Party members did not differ from nonmembers in attitudes toward political change suggests (as noted) that on these issues the vanguard role of the Party was to a large extent ephemeral. But it is also the case that at the time our survey was in the field, the Gorbachev reforms had not penetrated very deeply into the Party itself, that is, to the rank-and-file politically and geographically distant from the Moscow center. Gorbachev's advocacy of a reformed role for Party members (opening their operations to the scrutiny of glasnost, mobilizing non-Party people for greater participation in the workplace, and taking the lead in enlisting support for the complex of political reforms) did not reach very far below the higher echelons of power.

For example, in surveys carried out by the Academy of Social Sciences of the Party's Central Committee in 1986, 1987, and 1988, among the low-level Party officials who ran grassroots Party organizations, it was found that the reform process had "touched only the highest strata" of the Party. Very few had noticed any increase in the Party's attention to public opinion in the workplace, and an analysis of local Party summaries of operations showed little change from past practices (Toshchenko 1990). In fact, Party members failed actively to involve themselves or to change decisively—or even noticeably—their style and content of work, while the "informal organizations" and the competing political parties in process of formation increasingly structured the debate ("Pochemu otstaet Partia?" 1989).

Our findings on support for individual versus collective responsibility underline the conservative posture of Party members. Apart from age, education, and occupational prestige, Communist party membership is among the more influential variables in its association with the traditional collective ideology. Of Party members, 62% prefer the state to take responsibility for individual well-being, as compared to only 49% of non-Party people. Given this propensity for continuation of the ideological status quo, it is not surprising that Communist Party members as a whole are not in the forefront of those advocating more rapid change and greater political freedom to advocate change.

Education

A large literature indicates that more highly educated individuals are more tolerant of political deviance and more likely to engage in political participation. This suggests that the highly educated in the Soviet Union will favor reform, at least insofar as it concerns political democratization. Consistent with this, we find a positive relationship between education and support for political change (tau b = .17, gamma = .25). We also find that education has an equally strong impact on level of support for individual versus collective responsibility but that, interestingly, it also increases support for collectivism. Only 35% of those with higher education favor individual responsibility for social well-being, as compared to 51% of those with secondary education, and 58% of those with eight or fewer years of education (tau = −.15, gamma = −.26). Moreover, education is important: it lags behind only age and Central Asian ethnicity in its predictive power on support for political change and is the most important variable in predicting attitudes toward preference for state or individual responsibility for citizens' well-being. In regression analyses, the opposite signs for the relationships of education to our two dependent variables are maintained even after a large number of other variables are controlled. It is noteworthy that the relationship of education to support for collectivism is not consistent with the SIP data: among Soviet émigrés, Silver (1987) reports that the more highly educated are more likely to favor private/individual rights and solutions to problems than collective/state rights and solutions.[5]

Our interpretation of these differential relationships follows our theoretical discussion. Many observers have noted that the principal beneficiaries of the political changes instituted in the last few years were precisely the highly educated. Mobilizing this stratum for rapid political and economic change required the distribution to them of valued advantages. They were the recipients of both the material and the psychic gratification associated with glasnost: the foreign trips and goods and the ability to form new associations and acquire previously restricted information. What they apparently learned from this advantage in terms of their political attitudes was a strong propensity to favor the political freedoms represented in our scale of political change; but the changes they preferred were narrow and restricted to those that provided them with the benefits they sought—primarily the intellectual benefits that flow from political freedom.

Nevertheless, the issue should not be framed solely in terms of instrumental personal advantage. Many of the most educated are precisely those who believe in a reasoned fashion that social justice requires the strong hand of the state. They are concerned that irrespective of the record of success of this particular system, the state does bear the onus for the social welfare of its citizens. In particular, as the provision of a safety net for the elderly, the young, the infirm, and returning veterans proves illusory, calls for a return to a greater role for the state in assisting the dependent population grow stronger. Many of those who search for models for the post-Soviet future look to countries where the role of the state is greater than it is in the United States. Thus, the well educated are favorably disposed to political change that benefits themselves but also resist certain changes that may be ideologically incompatible.

Income

Income plays a significant role in increasing support for political change, although its effect is considerably weaker than that of education. The reforms thus far are associated in the minds of many citizens of the former Soviet Union with a worsening economy, in which only a few groups have managed to derive material benefits (Levada and Sedov 1990). The few groups include those who run cooperatives and those who conduct illegal or partially illegal economic activity. In this context, it is not surprising that those with lower incomes are more likely to be fearful of, and opposed to, reform as compared to the wealthier, although the differences are not extremely large. In fact, the economic effects of the changes initiated by the Gorbachev leadership disproportionately burdened those with lower incomes.

Economic problems are compounded for older people on fixed incomes. A Soviet study shows that the majority of people who retired in the 1960s live on less than 50 rubles per month. This is considerably below even the unrealistically low poverty line of 70–75 rubles per month at the time of our survey. As the world now knows in great detail, many goods are in such short supply that only barter arrangements or radically inflated prices on unofficial or illegal markets can supply necessities. At the time of our survey, food purchases accounted for at least half of all expenditures of family income ("Prozhitochny minimum" 1990).

Issues of structural unemployment were already of concern to Soviet citizens well before explicit debates on a market economy broadened the scope of expected insecurity. The economies of the Central Asian republics were particularly affected: in the first six months of 1989, only some two hundred thousand people out of the three million unemployed in Central Asia were able to get work ("Bez raboty sredy morya raboty" 1989). The Gorbachev administration had indicated that the reforms on which it embarked would be accompanied by a "safety net" for the needy population. For example, all military demobilization was supposed to take place in synchronization with increased availability of housing and other necessities. However, the Soviet population saw little concrete preparation of such a safety net. Given widespread poverty, political and economic change may have represented a potential threat, rather than an opportunity. In this context, the higher the income, the more the individual would be able both to withstand the dislocations of change and to benefit from the increased freedom it represented.

Curiously, however, income had no impact on attitudes toward individual versus state responsibility for social well-being. While one might expect that those who are less able to care for themselves would favor a system in which the state took on this responsibility, we do not find this to be the case. This might well reflect an opinion held by the poor that the state has not been successful at this enterprise.

Gender

Men are significantly more likely than women to favor political change. This finding is consistent with much other research, in both the West and the Soviet Union, on the greater political conservatism of women, their lesser political interest and participation, and their stronger support for and trust of political authority (Jennings 1983; Jennings et al. 1990; Jennings and Niemi 1981; Shapiro and Mahajan 1986; and Carnaghan and Bahry 1990). However, gender is unrelated to position on state versus individual responsibility.[6]

In the former Soviet Union, the issue of gender is in part a cultural/ethnic one, in which aspirations and levels of mobilization of women differ radically from the Muslim East to the European West. We find that in its effect on support for political change, however, gender operates independently of the cultural factors associated with geographic region. Nevertheless, other factors create rather different environments for men and women. In spite of both roughly equal chances for educational attainment and high levels of work force participation (though by no means evenly distributed across occupations and disproportionately found in feminized occupations), women are less embedded in political life and more isolated than men. Leisure time is unequally distributed; and given the relatively low availability of appliances and other domestic aids, women, most of whom work in the national economy and also bear the burden of domestic obligations, have less free time than their male coworkers. Again, as elsewhere, our data show males as significantly more likely to express interest in politics than females, a major factor in receptivity to change. Forty-five percent of males, but only 32% of females, reported that they were "very interested" in politics (tau c = −.14, gamma = −.22).

Occupation

In discussions of support for political change in their country, Soviet analysts often referred to occupational sector and status. "Bureaucrats" or the "bureaucracy" were often cited as the principal sources of obstruction to Gorbachev's reforms. Zaslavskaya (1990), for example, found the greatest resistance to change in the "directing apparat" (high-level bureaucrats), among people who work in the trade and communal services sector and among workers who are corrupt or severely downwardly mobile (*déclassé* is her term). She finds that certain others, whom she calls "quasi supporters," adopt an outward posture of support while disguising their real (negative) views toward political change; among these are people who work in the Communist Party apparatus, the state apparatus, and economic leaders.

On the distinctly supportive side, she finds those who have begun to participate in the new structure, such as employees and directors of cooperatives, leaseholders and others exercising new rights for individual production, workers who participate on the more flexible and remunerative group contract system, and finally, people who work on successful collective farms. Except for the last, all of these groups have already voted with their choice of job to accept change and depend on its continued expansion for their success. They are risk takers in a rapidly changing system.

In our survey, only those who work in the agricultural sector distinguished themselves by being less inclined to support political change than workers in any other branch of the economy.[7] This is consistent with findings in studies in many countries of the generally stronger conservatism of farmers and agricultural workers as compared to industrial workers. In Soviet society, agricultural workers were, by and large, not the risk takers whom Zaslavskaya describes. Even the notion of a "successful collective farm" is more often than not oxymoronic. The traditionalism of the agricultural sector and of rural life in general (as our findings about urban–rural differences in support for political change will show) was certainly a feature of Soviet political life, exacerbated by the isolation and disparities in development between that sector and others (Breslauer 1982; Hewett 1988).

The negative posture of the agricultural sector may reflect not only the traditional unwillingness of the least modern sector to change but also a certain skepticism about the reality and effects of reforms and of the ability of the agricultural sector to benefit from them as long as collective organizations survive and land is not fully privatized.

Urban or Rural Residence

We created a simple dichotomous measure in which cities of all sizes are classified as "urban," and rural and agricultural areas as "rural." Urban residents are greater supporters of political reform and democratization than are their rural counterparts (gamma = .28). This is consistent with numerous studies documenting the greater conservatism of those living in rural areas. Also (as we would expect), urban residents are more supportive of state responsibility for the social well-being of individuals, while rural residents are more likely to prefer a larger role for individuals (gamma = −.14).[8]

In terms of building support for the types of changes being undertaken in the former Soviet Union, these relationships reveal some of the difficulties when people do not see political reforms as unidimensional; that is, there is more support in the urban areas for political change in general, but this remains coupled with lingering support for state-oriented solutions to social and economic problems. In the rural areas, in contrast, there is less support for political change in general but more support for a movement toward greater individual responsibility.

Satisfaction with One's Life

Apart from education and age, life satisfaction is the only variable we have investigated whose effects survive simultaneous control for many other variables in multiple regressions for *both* of our dependent variables. For this reason—and because it plays an important role in some development literature—we included it in our analysis despite its different conceptual basis as compared to the social structural variables that constitute most of our independent variables.

Life satisfaction is often considered part of a cluster of cultural attitudes that are necessary for political democratization and economic change to take root (Inglehart 1990). At the aggregate national level, Inglehart demonstrates that life satisfaction is highly related to stability of democratic political institutions. In our Soviet data, however, higher satisfaction with one's own life resulted in *decreased* receptivity to change (tau b = −.12, gamma = −.17). That people who are satisfied with life want less change than the dissatisfied seems unremarkable. Reflecting on Inglehart's finding, however (which might be read to mean that the satisfied support democracy), we see that a different interpretation may be more appropriate: political change is less likely to come from the satisfied, whether they live in democratic or totalitarian systems.

On the other hand, we also find that individuals who were more satisfied with their own lives were more inclined to situate responsibility for social well-being with the *individual*, rather than the state (tau b = .10, gamma = .16). It is possible that people who were satisfied with their lives in the Soviet Union identified the source of their well-being in themselves or in their special abilities to exploit state programs, rather than more directly in any outputs they may have received from the state economy and social welfare programs that collectively represented the state-oriented ideology. The relationship between life satisfaction and preference for individualistic solutions is another (modest) indicator of the failure of state programs in the Soviet Union. Yet, interestingly, even in a state-run system, if one has found ways to cope and is therefore relatively satisfied, change is seen as less desirable.

TABLE 8

Regression Equations for Support for Political Change and Locus of Responsibility for Social Well-Being

	SUPPORT FOR POLITICAL CHANGE (POSITIVE = HIGH)			LOCUS OF RESPONSIBILITY (INDIVIDUAL = HIGH)		
INDEPENDENT VARIABLES	UNSTANDARDIZED COEFFICIENT	SE	BETA WEIGHT	UNSTANDARDIZED COEFFICIENT	SE	BETA WEIGHT
Central Asian ethnicity	−.491	(.065)	−.200	—	—	—
Age	−.192	(.028)	−.182	−.064	(.013)	−.139
Education	.183	(.038)	.127	−.106	(.018)	−.169
Gender (male high)	.452	(.117)	.098	—	—	—
Urban residence	.380	(.126)	.080	—	—	—
Life satisfaction	−.234	(.078)	−.077	.108	(.036)	.082
Income	.065	(.029)	.058	—	—	—
Georgian ethnicity	—	—	—	−.123	(.042)	−.079
Constant	1.485	(.403)		2.317	(.161)	
Multiple R		.397			.231	
R^2		.158			.053	
Adjusted R^2		.153			.050	
Significance of equation		<.001			<.001	

Note: All coefficients presented are significant at $p < .01$, except for income, which is significant at $p = .02$.

MULTIVARIATE ANALYSIS

A multiple regression equation (ordinary least squares) predicting support for political change confirmed that of the variables just considered, the most important were Central Asian ethnicity, age, gender, education, income, urban residence, and life satisfaction. (The multiple R is .40 and the adjusted R-squared is .15.) Table 8 gives the values for this equation. Working in the agricultural sector also has an impact but its coefficient is just short of significance, and the increase in the R-squared is insignificant.

For comparative purposes, Table 8 also presents an ordinary least squares regression for state versus individual responsibility for social well-being.[9] We are less successful with prediction of this variable (as would be expected with a one-item dichotomous measure). Including education, age, life satisfaction, and Georgian ethnicity (all of which make significant independent contributions to the equation), the multiple R is .23, and the adjusted R-squared is .05. Increasing age, higher education, and Georgian nationality increase the likelihood of preferring a state-oriented solution to social problems. Life satisfaction correlates positively with preference for an individualistic orientation. Occupational prestige and urban residence are other variables that are important to this measure but fall just shy of significance in the regression. A logistic regression of this question confirms education, age, Georgian ethnicity, and life satisfaction as the major influences.

As we have discussed, the regression coefficients show that both education and life satisfaction have opposite effects on our two dependent variables: education is associated with increased support for political change but decreased support for individualistic solutions, life satisfaction, with decreased support for political change but increased support for individualistic solutions. These changes of predictor variables' direction of influence on our two different measures of political reform demonstrate how the complexity of political change may lead the same people to react differently to its various aspects. Clearly, such issues need to be taken into account in developing measures of attitudes toward development, modernity, democratization, political change, and related complex concepts, and multi-item measures should be rigorously evaluated to be sure that all items are consistently related to each other.

CONCLUSION

Our findings about the multidimensionality of attitudes toward the complex of reforms introduced in the Gorbachev era have important political implications. That attitudes on these related subjects exist without significant associations with each other means that there is little systemic constraint on attitudinal lability. Major changes in support for particular aspects of political or economic reform can occur as change occurs in political or economic conditions.

Using our measure of support for modernizing political change, we find that its strongest proponents tend, though not without exception, to be the beneficiaries of those policies. Many of the supporters also fall into groups identified by cultural theory as "modernizing" groups. For example, the young have derived both psychic gratification from the policies and are looking at distinct career advantages. Most flexible in their outlook, and most able to change, they are the ones who are most likely to adapt successfully to a radically changed economic and political environment. They are better educated than their predecessors and better able to advance in a situation of much greater uncertainty. The college-educated, however, may still support a strongly interventionist state when contrasted to wholly autonomous individual responsibility not only because of the relative advantages they might have derived from it but also because of an attachment to the notion that (an improved version of) the state might do better as a guarantor of social justice than would the unfettered operation of a market.

Those with higher income have greater insulation and can better withstand the sharp rise in prices. Those at the low end of the status scale (in terms of education and income) are unconvinced that the much-publicized projected safety net for the disadvantaged will offset increasingly burdensome dislocations. Males are also relatively more advantaged, empowered, mobilized, and able effectively to take advantage of political change and to benefit from it. All of these characteristics, as well as urban residence, are also associated with greater participation and mobilization.

Ethnicity remains a key factor in attitudes toward political change. But for the largest ethnic group, contextual factors are also important. There are substantial differences between Russians in their home republic, the Russian Federation, and the many millions of Russians who live, for the most part, as minorities in other republics.

Our research thus provides support for cultural theories of the role of mass attitudes in political change, insofar as this relates to the groups that will support modernizing social change; but our interpretations stress that support also arises out of particular material benefits realized by some groups from social change.

The ethnic and sociodemographic differences we observed as the Soviet Union was collapsing create serious problems for democratizing reform in the newly independent successor states. These divisions are an inevitable part of the legacy of the former Soviet republics and may well be magnified under conditions of extreme economic and political uncertainty. The traditional source of mobilization—and the source in control of many of the instruments of mobilization—had in the past been the Communist Party. But members of the Party were not performing that function as both their influence and cohesion dissipated. To mobilize and forge the coalitions necessary for developing the institutions (including effective political parties with an adequate base of popular support) and habits of change, the range of beneficiaries must be expanded. As our data show, support for both democratic change and change in the scope of state control is tentative. Unless there are timely outputs—both economic and political—that alleviate the serious material problems, legitimate the leadership, and confirm both the efficacy and utility of procedural and normative change, the process of institutional development will be substantially impaired.

APPENDIX A: THE SAMPLE

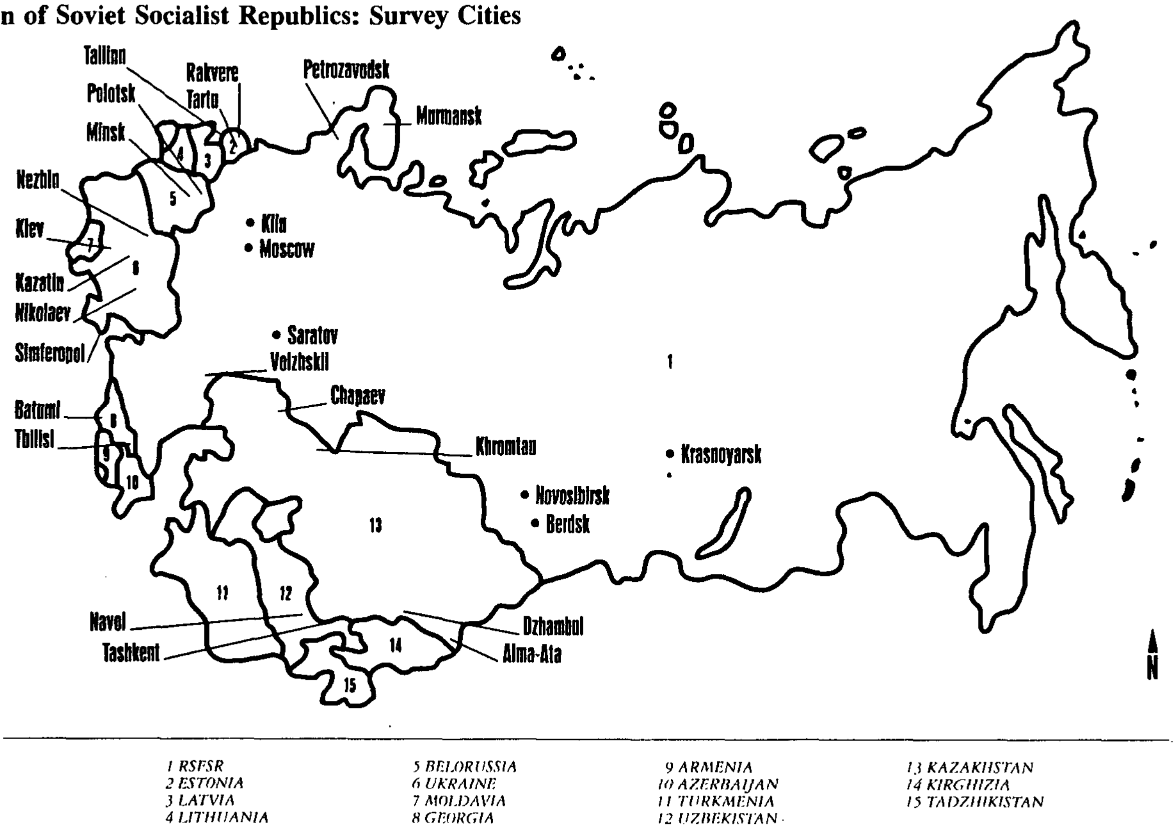
The sample was designed to represent the entire Soviet population, as well as the major nationality groups. The design was therefore clustered in seven of the 15 republics: Estonia, Belorussia, Georgia, Kazakhstan, Uzbekistan, Ukraine, and the Russian republic (RSFSR), which together contained 88% of the Soviet population. For the smaller republics, the sample uses clusters of republics, grouped according to cultural similarity and/or geographic proximity, to represent various regions of the nation. Thus, interviewing for the Baltic region of Estonia, Latvia, and Lithuania took place entirely in Estonia; interviewing for the region including Ukraine and Moldavia took place in Ukraine; interviewing for Georgia and Armenia took place in Georgia; and interviewing for Azerbaijan, Uzbekistan, Kirgizia, Tajikistan, and Turkmenia took place in Uzbekistan. Because of either large size or cultural dissimilarity, the Russian republic, Kazakhstan, and Belorussia were not grouped with other regions. Sampling within the very large Russian republic was further stratified by region, and separate samples were drawn from its five regions: East Siberia, West Siberia, the Northwest, Volga, and Moscow.

In order to achieve larger samples of important nationalities, we sampled disproportionately in Belorussia, Estonia, Georgia, and Kazakhstan. At the same time, achieved samples were somewhat smaller than planned in Ukraine and Uzbekistan. The final sample of 2,520 cases was therefore weighted to correct for the disproportional stratification. The weighted sample size is 2,006 and the number of cases in each regional area in the weighted sample accurately reflects the size of the population in each corresponding territorial region. All reported numbers of cases are based on the weighted sample size. The downsizing of the sample occurs because a weight of 1 was assigned to respondents in regions sampled with probability proportionate to size and oversampled groups were then downweighted against this standard. This procedure is preferable to upweighting the former group, because it does not bias tests of significance, based on the number of cases in the sample.

In each republic, interviewing took place in the capital, in one or more other midsized cities, and in some smaller rural and semirural areas. Given the lack of infrastructure for survey interviewing and difficulties of transportation, some interviewing sites were selected on the basis of geographic proximity to the twenty-six regional centers of the survey organization, availability of field supervisors and interviewers, and so on. Within these major sites, the design specified random selection within strata defined by age and sex, based on census information about the distributions of these characteristics within each regional cluster. In each sample location, names were chosen randomly from lists maintained in the government's population registration office. Name, sex, and age are noted on each individual's registration

FIGURE A-1

Union of Soviet Socialist Republics: Survey Cities


Tallinn
Rakvere
Petrozavodsk
Polotsk
Tartu
Minsk
Murmansk
Nezhin
Kiev
Kalin
Moscow
Kazatin
Nikolaev
Simferopol
Saratov
Volzhskii
Chapaev
Batumi
Tbilisi
Khromtau
Krasnoyarsk
Novosibirsk
Berdsk
Navoi
Tashkent
Dzhambul
Alma-Ata
N
1 RSFSR
2 ESTONIA
3 LATVIA
4 LITHUANIA
5 BELORUSSIA
6 UKRAINE
7 MOLDAVIA
8 GEORGIA
9 ARMENIA
10 AZERBAIJAN
11 TURKMENIA
12 UZBEKISTAN
13 KAZAKHSTAN
14 KIRGHIZIA
15 TADZHIKISTAN

Note: Rural areas are not shown.

card, and interviewers sampled systematically to achieve the required numbers of cases in each stratum. The map presented as Figure A-1 shows the urban locations of the survey; the survey also included a large number of rural sites not shown on the map. This was the first Soviet survey to include such large numbers of rural and ethnic minority respondents. Since 34% of the Soviet population lives in rural areas, this is an important component of a representative sample. The interview was translated into several of the national languages (Estonian, Kazakh, etc.) and conducted, when appropriate, by conationals who were native speakers of those languages.

Since the sites themselves were not selected by strict probability methods, an exact estimate of sampling error is precluded; however, an educated guess is that it might be about 6%–7% for estimates based on the entire sample (basically doubling the sampling error for a random sample of this size). Given current practice even for samples that do not meet all the criteria for probability samples, we do occasionally report significance tests. We intend these as a general guide to the reliability of the data, rather than as rigorous tests of significance.

Even though we do not adjust the sample for any characteristics other than geographic region, it matches the Soviet population reasonably well on a variety of demographic characteristics (see Table A-1). We have the usual overrepresentation of well-educated people. It is therefore not surprising that the sample somewhat underrepresents the manufacturing sector and somewhat overrepresents people in education and science.

Prior to fieldwork, regional survey center directors came to Moscow for training on our instrument and procedures; and they, in turn, recruited and trained interviewers in their regions. Interviewers were paid almost twice the usual rate, so that the most experienced and best-trained could be hired. Armed with the names and addresses of sampled persons, interviewers returned up to three times to locate designated respondents. Virtually no interviewers had access to cars; and given the large size of the cities and apartment blocks and the usual difficulties of urban transportation, three callbacks in many cases represented a genuinely heroic effort. Following three unsuccessful callbacks, local procedure entailed substitution of a new name, a procedure also used in the United States by many commercial polls. Thus, the concept of a "response rate" does not seem to exist; nor, unfortunately, are systematic records kept of which respondents were substitutes.

Interviews lasted, on the average, about one-and-a-half hours. Each respondent was assured of confidentiality and anonymity; and, following the interviews, the names of respondents were separated

TABLE A-1

Population and Survey Sample Comparisons (%)

	POPULATION	SAMPLE
Age		
16–19	8.4	10.0
20–29	21.5	22.3
30–39	21.3	19.5
40–49	13.8	16.1
50–59	15.5	16.7
60+	19.6	15.3
Gender		
Male	45.7	46
Female	54.3	54
Marital status (men)		
Never married	20.6	21.9
Married	71.5	74.1
Widowed	2.6	1.1
Divorced	4.7	2.9
Marital status (women)		
Never married	14.0	18.0
Married	60.6	60.6
Widowed	17.4	11.3
Divorced	7.5	10.1
Education		
Primary or incomplete secondary	38.8	19.1
Secondary, general	30.5	23.9
Secondary, special	18.2	29.3[a]
Incomplete higher	1.7	4.8
Complete higher	10.8	22.8
Employed	61.9[b]	70.8
Occupational sector		
Indust., construction, transp., communication	48.8	40.6
Education, culture, science	13.6	17.9
Trade, public catering	8.1	5.8
Housing, service	4.0	6.6
Health care	5.9	5.3
Agriculture	18.3	18.7

Note: The sample data are weighted only by geographic region. Population data include persons aged 16 and over, except for education, for which the data given are for ages 15 and over. For the age distribution of the population, the size of the group aged 16–19 is estimated as 40% of population aged 10–19, which is the category used in the source document.

Sources: Population data are from *Narodnoe khozyaistvo SSSR v 1989g* [The economy of the USSR in 1989] (Moscow, 1990) and a USSR State Committee on Statistics press release of January 1990. We should like to thank Vladimir Treml for his assistance.

[a] Includes both secondary special and secondary professional–technical.

[b] Soviet sources cite 127.033 million persons employed in the officially defined national economy. To that we add an additional 1.5 million estimated to be employed in the private sector. Excluding the military, then, we estimate 61.9% of the population of 207.729 million (as of 1989) to be in the civilian labor force.

from their interviews. Validity tests included post card verification of interviews, callbacks on a small percentage of respondents, and name recognition questions that included a nonexistent television personality. Carbonless copy paper enabled both the Soviets and Americans to have an "original" of all interviews, and both sides conducted independent coding operations. The data used in this report are all based on the American coding operation and weighting procedures.

During the data collection, we visited a number of survey sites. In general, the dedication of the staff was extremely high, stimulated by a sense of the importance of the scientific enterprise and the conviction that representative public opinion data was an important political input in the democratization of the nation. It was our impression from informal conversations with many interviewers that there were very few outright refusals, so that any bias in the sample is introduced almost entirely by the sampling frame per se and the not-at-home factor. Although the sample does not in all important respects measure up to the highest Western standards, it is probably one of the best national samples achieved to date in the Soviet Union.

APPENDIX B: QUESTIONS USED FOR DEPENDENT VARIABLES

Q18. Some people think that to solve our most pressing problems it is necessary to make decisive and rapid changes, since any delay threatens to make things worse. (R)

Others, on the other hand, think that changes should be cautious and slow, since you can never be sure that they won't cause more harm than good. Which of these points of view are you more likely to agree with?

Q22. Below are some widespread but contradictory statements relating to problems of the development of our society. Which would you be most likely to agree with?

a. The state and government should be mainly responsible for the success and well-being of people.

People should look out for themselves, decide for themselves what to do for success in life. (R)

b. The state should provide an opportunity for everyone to earn as much as he can, even if it leads to essential differences in people's standard of living and income. (R)

The state should do everything to reduce differences in people's standard of living and income, even if they won't try to work harder and earn more.

c. Strikes, spontaneous demonstrations, political meetings and other forms of social protest are completely acceptable methods of mass conduct and an effective means for solving social problems. (R)

These forms of protest are undesirable for society, they should be avoided in favor of more peaceful, traditional and organized methods of solving social conflicts.

d. To improve things in our country people should be given the opportunity to say what they want, even if it can lead to public disorder. (R)

Keeping the peace in society should be the main effort, even if it requires limiting freedom of expression.

Q29. In the coming elections for local soviets, should we elect deputies from among several candidates, as we mostly did in the spring elections? (R)

Or is it better to avoid the conflicts these elections generated and go back to the old system of voting?

Notes

The design of this study was a joint product of the authors, together with George Gerbner, of the University of Pennsylvania, and Boris Grushin and Nikolai Popov, both of whom were at the time affiliated with the All-Union Center for Public Opinion Research. Grushin is currently the director of Vox Populi, a private public opinion polling organization based in Moscow. We wish to express our gratitude for the cooperation and contributions of these individuals to the study design, as well as to T. I. Zaslavskaya, director of the All-Union Center, which carried out the field work. Funding for this project was provided by the John and Mary R. Markle Foundation, Rockefeller Family and Associates, International Research and Exchanges Board, Coca Cola Corporation, and American Express. We also wish to thank Michael Sweat for his capable assistance with data management and computer support and Paul R. Abramson of Michigan State University for his helpful comments on an earlier version of this paper. The two of us share equal responsibility for the work.

1. "Maintaining order in the nation" and "protecting free speech" are components of Inglehart's well-known measure of "materialist versus postmaterialist values" (see, e.g., Inglehart, 1990). Unfortunately for comparative purposes, that measure asks people to rank these and other values, so that direct comparisons with our measure are not generally possible. In addition, most of the published accounts using Inglehart's measure report only composite results for the overall scale, which includes many values not germane to our analysis.

2. Recent relevant European experience is worth noting. Support for unconventional means of political participation declined in both West Germany and the Netherlands between 1974 and 1979–81 (Jennings, et al. 1990).

3. For purposes of this discussion we include Kazakhstan in Central Asia. Rumer (1989) summarizes the distinctions between the four- and five-republic definitions of Central Asia.

4. Investigators working on this project collected data from 1970s émigrés from the USSR and generalize it to the western urban population of the Soviet Union.

5. While Silver used a three-item measure of state versus private control of medical care, heavy industry, and agriculture and our own measure is a single question about state versus individual responsibility for the "success and well-being of people," the core concepts involved in both measures seem similar. An intriguing possibility to explain the discrepancy in results is that the émigrés may be an ideologically self-selected group in which background characteristics relate differently to basic social and political attitudes. Since emigrants are generally thought to be less risk-averse than nonmobile individuals, it makes sense that the most advantaged among them would favor individualistic solutions to social problems. Among the nonmobile, on the other hand, socialization might increase support for regime norms (in this case, state-oriented solutions to social and political problems) among the best-educated.

6. Our finding is again at odds with the Soviet émigré project, since Carnaghan and Bahry report that women emigrants were considerably less supportive than men of state control. Again, it is possible that the difference is caused by emigrant versus resident status. Possibly, female emigrants are more resentful about the practical difficulties of life for women in the Soviet Union.

7. In our sample, the agricultural sector includes not only farmers but also animal husbandry and other technical and scientific specialists, machine operators, administrative and clerical personnel, and all others who support the farm sector, whether in state or collectivized agriculture.

8. We also used more refined measures of city size with no noticeable improvement in prediction. Very large cities in the more conservative central and eastern areas of the country are not as supportive of change as are smaller cities in the west, creating some curvilinearities in the relationships. Overall, therefore, because the largest differences are between the rural areas and cities of any size, the simple urban–rural dichotomy is more successful than ungrouped city size or any more refined grouped measure. For example, for political change, the gamma is .18 with unrecoded city size and .20 with a collapsed measure with six values. For Q22a, the analogous values are −.09 and −.10.

9. While logistic regression is preferred for dichotomous dependent variables, with very even distributions, such as the 49–51 split we have in this case, ordinary least squares generally yields the same results.

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ECONOMIC TRENDS AND GOVERNMENT SURVIVAL IN WEST EUROPEAN PARLIAMENTARY DEMOCRACIES

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In this study, I investigate the linkage between trends in key economic indicators (inflation, unemployment, and growth in gross domestic product) and government survival in 16 postwar European parliamentary democracies. The partial likelihood method, which allows for variation in indicator values over the lifetimes of individual governments, constitutes the basic analytic tool. The findings reveal overall causal roles for both inflation and unemployment, as well as important differences in these roles between socialist and bourgeois governments and between pre-oil crisis and post-oil crisis eras. Most significant, the introduction of these indicators to the analysis helps to resolve the debate between two rival explanations of governmental stability, the bargaining complexity hypothesis and the ideological diversity hypothesis, in favor of the latter.

The study of government stability in parliamentary regimes has been enlivened within the last decade by a debate over the fundamental nature of the phenomenon and how it should be investigated empirically. The standard approach, as in much of the social sciences, is the causal one; it relates the durations of governments to a variety of properties of those governments or their parliamentary environments, usually by means of regression analysis. Critics of the causal approach have argued, however, that it mischaracterizes the underlying process: governments do not fall because they lack properties normally associated with longevity, they fall when challenged too severely by events or developments occurring in the outside political, social, or economic environments (Browne, Frendreis, and Gleiber 1984, 1986, 1988). Since events of this sort are essentially random with respect to individual governments, they maintain that the task at hand is not to associate characteristics with government durability but to model the randomness of government durations.

The "events" theorists clearly have a point: to treat a government's duration solely as a function of a set of characteristics most or all of which are fixed by the time the government assumes office is excessively deterministic. Some allowance must be made for developments that occur during the government's lifetime. Yet to expunge causal factors from the analysis totally—to treat durations as purely a function of random outside events—seems even more unrealistic. If British governments normally remain in power longer than their Italian counterparts, for example, it is surely due in part to systematic differences between the two systems (less party–system fragmentation, more single-party majority governments, etc.), not simply because British governments face fewer or less difficult events.

These observations suggest that an optimal strategy for advancing research on this topic would be to combine the insights of both perspectives in some fashion. Since the events thesis of random collapses implies that government durations are distributed according to a negative exponential distribution (Browne, Frendreis, and Gleiber 1986), King and his colleagues (1990) recently proposed a sensible unification: a model that treats government durations as a function of a set of independent variables and an underlying exponential distribution. In their "unified" model, the systematic element in government durations is taken up by the independent variables; anything that cannot be explained by those variables is relegated to the exponential distribution. Estimated by the maximum likelihood method, the model possesses the important property of accounting for all significant intercountry differences in durations, thereby resurrecting comparative or cross-national analysis from the limbo to which the system-specific approach of the events theorists had banished it.

Although the unified model shifts the focus back onto the systematic factors (events become, in effect, part of the error term), it could be argued that it adheres too closely to the events approach in two respects. First, it accepts rather uncritically the appropriateness of an exponential specification, an assumption that Easton and I (Warwick and Easton 1992) found to be unwarranted.[1] Second (and more important for the purpose at hand), it implicitly endorses the events theorists' dichotomization of relevant factors into fixed characteristics and totally random events. The weakness with this categorization is that it omits a third major alternative: factors which vary in a nonrandom fashion over the lifetimes of governments. Foremost among these factors in democratic regimes is the changing state of the economy.

I shall attempt to relate evolving economic conditions—as measured by inflation, unemployment, and economic growth indicators—to government survival in West European parliamentary systems in a systematic empirical fashion. Although there have been numerous efforts to associate economic indicators with opinion poll standings or with electoral outcomes, attempts to establish a linkage with the survival of governments are relatively rare. A standard property of these attempts, moreover, is the characterization of economic conditions during each government by means of average indicator values (e.g. Budge and Keman 1990, 176–82; Robertson 1983b, 1983c), thereby rendering static an analysis that is intrinsically dynamic in nature.[2] There is no need for this compromise. Event history analysis—the broad class of techniques of which King and his colleagues' exponential model is an exemplification—provides ways to incorporate variables whose values change over the duration of individual cases directly into the analysis. I shall exploit this facility in order to determine whether—and under what conditions—changing economic circumstances bring about government terminations in parliamentary regimes, particularly when other relevant factors are taken into account. In so doing, I aim to fill a major gap in the empirical explanation of government stability in West European parliamentary contexts.

METHODS AND DATA

All event history models have in common the objective of explaining the "hazard rate" or rate of termination inherent in some phenomenon. Thus, a concern with durations may frame the theoretical issue; but it is typically the hazard rate that focuses the empirical analysis. (The two concepts are intimately linked, for high hazard rates mean short durations, and vice versa.) The hazard rate is generally taken to be a function of two components: (1) a set of independent variables or "covariates," which are related to the hazard rate in a log-linear fashion, and (2) a "baseline" hazard rate, which represents its underlying behavior. In mathematical form,

$$\lambda(t) = \exp(\mathbf{x}'\boldsymbol{\beta})\lambda_0(t),$$

where λ is the overall hazard rate, λ_0 is the baseline hazard rate, and $\mathbf{x}$ is a vector of covariates with associated coefficients $\boldsymbol{\beta}$.[3]

An important feature of this expression is that it allows the hazard rate to vary with duration time t. In fact, the assumptions one is willing to make concerning time dependency determine the type of model to be employed. An exponential model such as King and his colleagues' is defined by the relatively simple assumption that the baseline hazard is constant over the course of a case's existence; but a wide variety of more complex specifications are possible. Since there is evidence that complexity of this sort (known as "duration dependency") may characterize government termination rates (Warwick and Easton 1992), the data analysis undertaken here will employ Cox's (1972) partial likelihood (PL) technique, which, alone among event history methods, requires no specification of the baseline hazard function.[4]

Although the PL technique is itself complex, its essence can be conveyed in a relatively straightforward fashion. The basic idea is that the durations of all cases are ordered from shortest to longest, and then the covariate values for each case are compared with those for all cases that outlived it. If, say, the presence of covariate X leads to government terminations, one would expect that a case that collapsed after T days in office would have, in general, a higher X-value than the governments that survived beyond T days. When time-dependent covariates (i.e., covariates that take on different values over the lifetimes of individual cases) are involved, the PL technique compares, for the case that terminated at T, its X-value at that time with the X-values at the same point in the durations of all surviving cases. The amount of data involved in these comparisons is potentially very large; but the data collection task can be made easier if the values of time-dependent covariates are recorded for a limited number of time points, and intermediate values estimated, where appropriate, by means of linear interpolation. This is the strategy I shall follow.[5]

Apart from its capacity to handle time-dependent covariates, event history analysis provides a felicitous solution to a problem that has bedeviled previous work on government stability, namely, what to do with cases that end in a theoretically uninteresting way—especially governments that end with regular elections. These cases fit neither the events framework (since regular elections are not random events) nor the causal framework (since no properties of governments or their supporting parliamentary milieux are involved). Event history analysis simply "censors" these cases, that is, treats the true durations of these governments statistically as at least as long as the recorded durations. For the case of a government that ended with the expiration of its four-year legal mandate, this amounts to assuming that its duration, in the absence of this legal impediment, would have been not less than four years but may well have been longer.[6]

The censoring procedure may also be applied to other extraneous terminations, such as the death of a prime minister or a pro forma resignation required for technical reasons. Unfortunately, there are no statistical criteria for determining which circumstances merit this treatment, so the choice falls to the judgment of the researcher (Warwick and Easton 1992). In this study, cases are censored if their terminations (1) resulted from the death or illness of a prime minister, (2) were necessitated by legal or constitutional requirements, (3) had not occurred by the end of the observation period (31 December 1989), or (4) were brought about voluntarily within one year of elections scheduled before they took office. The last category is constructed, following King and his colleagues (1990), to allow for the possibility that governments might voluntarily dissolve themselves in advance of approaching elections in order to maximize their electoral advantage. As with the other categories, it is designed to capture situations where a government termination occurs even though its viability was not under challenge at the time.

One of the advantages of the censoring solution is that it accommodates the common practice among researchers of treating governments that survive elections and continue on in office as having been "terminated" by those elections. This convention has been widely adopted because elections change the parliamentary party configuration and because they involve a different criterion for survival—popular success, rather than the maintenance of parliamentary support or coalitional unity. With censoring, no artificial truncation of durations is entailed in adopting this sensible rule. Other events defined in this study as marking the end of a government include parliamentary defeats, changes in the prime minister or the party composition of the government, and government resignations (regardless of the composition of the succeeding government).[7] As in the case of regular elections, the censoring mechanism is used to adjust for situations where the termination seems more formal or technical than real.

One area in which the design of this study differs from many of its predecessors is in the conceptualization of a government. A government is here deemed to be composed not only of the parties who assume cabinet portfolios but also of the parties who formally or openly declare their support for the cabinet as well. The motivation for the expansion in the definition of a government to include supporting parties is to capture the true, or effective, governing coalition. It is noteworthy in this regard that in two-thirds of the cases where such parties can be identified, their support is necessary to create a parliamentary majority. Nevertheless, any relationships that emerge using this definition will also be tested on other government definitions, including the cabinet-only version, to determine whether it affects the empirical findings to any significant degree.

In order to facilitate the sequencing of durations that underlies the PL method, duration has been recorded in days, rather than the more usual months. The values of the time-dependent covariates are, accordingly, also estimated to the nearest day. With respect to the rate of increase in gross domestic product, which measures overall economic growth, this estimation process simply involves attributing an annual value to each day of the year in question. For inflation and unemployment, however, better estimates can be obtained from monthly figures than from annual averages.[8] The method used here derives daily values of these indicators from linear interpolations of their December monthly values, which raises two basic questions: Why choose December values? and Are interpolated values based upon one month per year sufficiently accurate?

Although, in principle, any month could have been chosen as the basis for the interpolations, December values possess the important property of being very close to annual values. The coefficients of .92 and .97 generated when annual values of inflation and unemployment, respectively, are correlated across all countries with their corresponding December values suggest that the December values are rarely idiosyncratic. In addition, the slope coefficients of .93 and .92 produced when the annual values are regressed on the December values indicate that December is broadly representative of the year as a whole (a perfect match would be reflected in a coefficient of unity). The effect of interpolating December values is therefore to register the trends in the data at a level that is representative of the entire year, eliminating in the process any distortions due to seasonal fluctuations (which are especially likely to afflict unemployment rates).

The smoothening effect of interpolation also removes a certain degree of month-to-month fluctuation that is normally present in economic data. If these fluctuations represent random "noise," this smoothening confers a significant added benefit (especially since the PL model does not incorporate a random disturbance term). It is possible, however, that the erratic high values that may occur in any month are what induce government collapses. If so, the data would underestimate the phenomenon. To test for this possibility, inflation was coded in two versions: the actual monthly rate and a pseudo–monthly rate constructed from the interpolated daily values. When the relationships of the two variables with the rate of government termination were tested by means of the PL method, the pseudo–monthly rate proved to have a noticeably stronger effect.[9] This finding suggests that politicians react not to the bad figures of a single month but to underlying trends extending over a period of several months—trends more clearly evident in the interpolated values.

The data collected for this study cover 16 West European parliamentary democracies from the resumption of normal politics after World War II until the end of 1989.[10] Parliamentary party strengths, the cabinet compositions of governments, the existence of support parties, and the termination categories used for censoring are all derived primarily from *Keesing's Contemporary Archives* (1944–90). With respect to the possible presence of support parties, other sources, (e.g., Browne and Dreijmanis 1982; Strom 1990; Swaan 1973; and various country-specific works) were also consulted. For maximum comprehensiveness, the data set incorporates all governments in the relevant time frames, including noninvested governments, caretaker governments, and single-party—as well as coalition—governments. Where exclusions occur, they principally involve the time-dependent covariates. Thus, unemployment rates are not available for the French Fourth Republic or Iceland and are scanty at best in the early years for most other countries. In addition, in several countries the method of calculating rates or index numbers is not consistent across the entire postwar period, producing values that are noncomparable. When changes in rates are the object of investigation, the loss of cases is not great (since only values spanning a transition in method have to be excluded); but when the rates themselves are being examined, the entire earlier series must be excluded, reducing the number of cases appreciably. The situation with respect to the other two indicators (the consumer price inflation rate and the gross domestic product growth rate) is not nearly so severe; but the consumer price data are less precise before 1964, when index values were given in whole numbers only.[11] Details on the sources of these data are given in Appendix A.

First, I shall examine the basic time trends in the economic indicators over the postwar era and test several versions of the indicators for their connection to rates of government termination, using the partial likelihood method. The analysis will then be broadened to incorporate the effects of other potential causal factors. As we shall see, economic indicators not only continue to demonstrate a causal role once other factors are brought into play but are instrumental in the evaluation of the relative merits of the bargaining environment and ideological diversity interpretations that have lately become major rivals in the explanation of government stability. Finally, I shall consider the success of governments of various ideological complexions (socialist, bourgeois, and mixed) in coping with inflation and unemployment; relate these records to their survival in office; and test the hypothesis that economic factors have had a greater impact on government stability in the more troubled economic climate prevalent in Europe since 1973.

ECONOMIC INDICATORS AND GOVERNMENT SURVIVAL

Before beginning the data analysis proper, it is useful to have in mind a general impression of the behavior over time of the three economic indicators: the consumer price inflation rate, the unemployment rate, and the rate of growth in the gross domestic product. In Figure 1, the time trends of these indicators are presented for the period 1950–89. These trend lines are based upon weighted averages of annual values of the indicators for the 16 countries of the sample, with the populations of the countries comprising the weights. The starting point chosen was 1950, because of the paucity of data before that time.

The trend lines pictured in Figure 1 indicate that after an initial bout of high inflation, both inflation and unemployment established and maintained relatively low levels throughout the 1950s and 1960s. In the 1970s, inflation rose to much higher levels and experienced two peaks (corresponding to the oil "shocks" of 1973–74 and 1979), then declined dramatically in the 1980s. The rise in unemployment in the 1970s was much more gradual, reaching an apex only in the mid-1980s before beginning a substantial decline. As for economic growth as measured by real increases in gross domestic product, it oscillates much more frequently and regularly than do either of these indicators but nonetheless reveals an overall tendency to decline over the period covered by the graph.

Although these trends contain few surprises, they do suggest that there is no simple trade-off between inflation and unemployment: both may be high or low at the same time. It is possible, therefore, that the two indicators have effects on government survival that are independent of each other. Conversely, there appears to be a tendency for higher levels of inflation and unemployment to be associated with lower gross domestic product growth rates and vice versa, suggesting that the latter may not convey new information about economic performance.

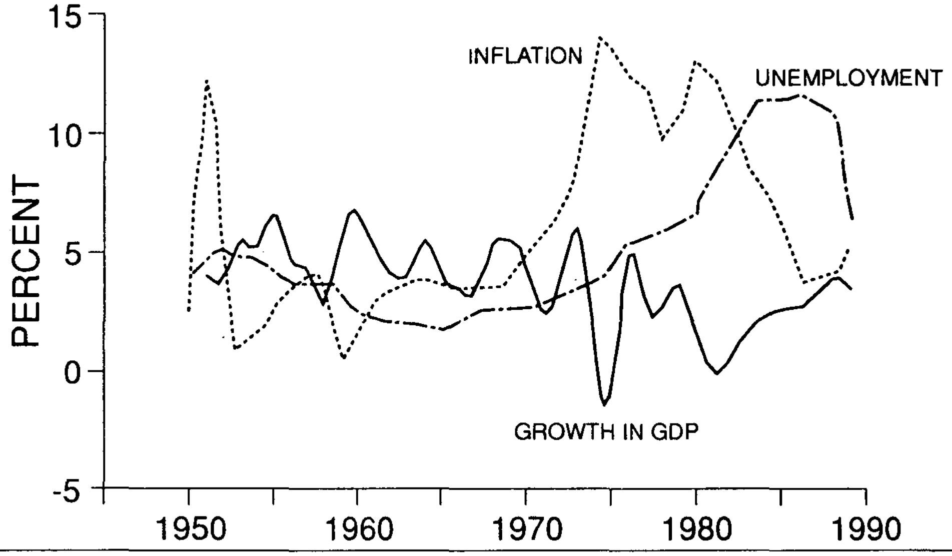
The hypothesis that economic trends are related to government survival receives a preliminary PL examination in Table 1. In interpreting the results listed in the table, it is important to bear in mind that since the model specification is log-linear, the effect of a one-unit change in a covariate on the rate of government termination is obtained by exponentiating its PL coefficient. For example, the coefficient of .06 attributed to the unemployment rate indicates that a 1% increase in unemployment is associated with an increase in the hazard rate of 6.2%, since exp(.06) = 1.062. It should also be noted that the procedures reported in Table 1 were performed with majority status (a dichotomous variable separating majority and minority governments) included as a control. This was done to prevent the possession of a parliamentary majority (or the lack of it) from masking the roles of economic covariates.[12]

The top of Table 1 explores the impact on government survival of four different measures of inflation: (1) the inflation rate, or percent increase in the consumer price index over the preceding year; (2) the change in the inflation rate over the preceding year; (3) the proportional change in the rate over the preceding year; and (4) the absolute value of the change over the preceding year. The last-mentioned category is included as a test of the hypothesis that economic change per se (regardless of whether favorable or unfavorable) brings about government instability. The results presented in Table 1 indicate that neither this nor any of the other proposed linkages finds support at the .05 level of significance (two-tailed tests), although that involving the inflation rate itself is sufficiently close that it should not be rejected out of hand. The direction of these relationships is as one would expect: high or increasing inflation rates are associated with higher hazard rates and thus a greater probability of government termination.

A very different picture emerges from the middle of the table, which contains results for the corresponding measures of unemployment. Because of the aforementioned noncomparability of data series, the unemployment rate itself is taken only from the most recent data series. The measures involving changes in unemployment rates may appropriately include earlier series based upon different counting methods, provided appropriate transitional situations are removed; this has the effect of increasing the number of cases modestly. The results indicate that the change in unemployment over the preceding twelve months is the most closely related to government survival, although all versions reveal relationships that are statistically significant or nearly so. Unemployment,

FIGURE 1

Average Annual Inflation, Unemployment, and GDP Growth Rates in 16 European Countries



Sources: Consumer prices from IMF; unemployment from UN; gross domestic product from OECD.

no matter how it is conceived, seems to be associated with higher rates of government termination.[13]

The final section of Table 1 shows the relationships with government termination of the analogous versions of growth in gross domestic product. Of the four versions, only the change in the growth rate from the preceding year shows any noteworthy effect. The negative sign associated with this effect indicates that increases in the growth rate of the economy are associated with a lowering in the rate of government termination, that is, with greater governmental stability. However, even this relationship falls well below the .05 level of significance.

The results reported in Table 1 provide some indication that economic trends (especially unemployment) are indeed associated with government survival in West European parliamentary democracies. This assessment accords with Robertson's (1983b, 1983c) earlier findings that the duration of governments is significantly related to average levels of

TABLE 1

Partial Likelihood Analyses of the Effects of Various Economic Indicators on Government Survival

COVARIATES	PARTIAL LIKELIHOOD COEFF.	T-VALUE	NUMBER OF CASES
Inflation			
Inflation rate (%)	.013 (.008)	1.73	305
Change in rate over prec. year	.024 (.018)	1.31	292
Prop. change over prec. year	−.003 (.006)	−.61	292
Abs. change over prec. year	.015 (.016)	.91	292
Unemployment			
Rate (%) (recent series)	.060 (.018)	3.35	193
Change in rate over prec. year (all series)	.235 (.055)	4.26	232
Prop. change over prec. year (all series)	.414 (.166)	2.50	232
Abs. change over prec. year (all series)	.124 (.065)	1.91	232
Gross domestic product			
GDP growth rate	−.021 (.028)	−.77	305
Change in rate over prec. year	−.036 (.024)	−1.48	296
Prop. change over prec. year	.001 (.001)	.77	296
Abs. change over prec. year	.011 (.035)	.32	296

Note: All coefficients are derived from partial likelihood analyses that include majority status as a control variable. Standard errors are in parentheses. Daily values for the inflation and unemployment covariates are interpolated from December monthly data; gross domestic product daily values are taken from annual figures.

unemployment—but not to inflation—in a more limited set of countries. The conclusion, of course, assumes all other factors equal; we cannot place confidence in it until the linkages have been shown to persist even after other relevant causal factors have been taken into account.

The analyses to follow will utilize the two indicators that showed the most promising results in Table 1: the inflation rate and the change in the unemployment rate. Not only is each of these measures more closely related to government survival than are the alternative measures of the same economic trait (controlling for majority status), but further analysis reveals that the presence of either of these measures in a PL model precludes a significant role for any of its alternatives. This suggests that the alternative indicators have relatively little to add to the statistical explanation of government survival.[14] This selection criterion is not foolproof; but in the absence of any political theory linking specific indicators to government survival, it is probably the most appropriate way to proceed at this stage.[15]

BARGAINING COMPLEXITY, IDEOLOGICAL DIVERSITY, AND THE ECONOMY

The goal of assessing the relative importance of economic conditions vis-à-vis other possible causes of government stability is complicated by the bifurcation of the latter along two very different interpretative lines: the *bargaining environment* thesis (King et al. 1990; Laver and Schofield 1990) and the *ideological diversity* thesis (Budge and Keman 1990; see also my earlier work, Warwick 1979). The bargaining environment interpretation views instability as the result of the absence of a stable core in a Euclidean ideological space—which, expressed less technically, means that coalition governments fall apart when at least one member believes that it can realize its policy goals more readily in a (viable) alternative coalition. A complex bargaining environment (one with many parties scattered throughout a large, multidimensional ideological space) increases the likelihood that these alternatives will exist. The ideological diversity interpretation, in contrast, sees government terminations as emanating principally from ideological or policy differences within governments, regardless of the availability of alternatives. The test of whether economic conditions matter for government survival therefore involves determining whether they continue to play a significant role once variables representing these two perspectives are introduced into the analysis; in the most optimistic scenario, this test will help to determine which perspective better fits the available evidence.

The main empirical support for the bargaining environment thesis comes from King and his colleagues' seven-covariate unified model. The four dichotomous covariates in that model indicate whether or not each government commanded a legislative majority, exercised its functions in a caretaker (nonpartisan) capacity, was the first to be formed after an election, and was obliged to undergo a formal vote of investiture. Bargaining complexity is conveyed by the model's three nondichotomous covariates: the number of failed formation attempts preceding the government's formation, the fractionalization (fragmentation) of the legislative party system, and its polarization as indicated by the proportion of seats held by "extremist" (antisystem) parties.

The theoretical role of these bargaining environment indicators varies considerably. A high number of formation attempts suggests a legislative situation rich in coalitional alternatives but gives no indication of the source of this condition. That source is more clearly identified with the system's fragmentation: other things being equal, more parties of significant size mean more viable alternative governments. Whether the presence of antisystem parties augments bargaining complexity is not so evident, however, given that these parties are typically excluded (by the prosystem parties, if not by themselves) from the business of bargaining over coalitions or participating in governments.

The ideological diversity thesis engages the debate precisely on this point. If antisystem parties are "noncoalitionable," as Laver and Schofield admit (1990, 200–201), their presence in force in a legislature should be associated with a lessening of coalitional alternatives and hence with greater government stability—the very opposite to what King and his colleagues found. Granted, a strong antisystem presence may be indicative of an ideologically divided and hence more complex party system. But the principal consequence of that polarization may be not a greater likelihood of attractive alternative coalitions, but rather the formation of governments from among prosystem parties that are themselves highly divided ideologically. These divisions may in turn predispose governments to early collapse. On this reasoning, one would expect ideological diversity within governments (provided it can be measured adequately) to displace the bargaining environment indicators in the explanation of government stability.

My previous research on government stability has concentrated on this measurement task (Warwick 1992[a]). The results of these efforts are three measures of ideological diversity, each significantly related to government stability. The first measure, which assesses the range of each government in a left–right dimension, is constructed from three "expert" assessments of party positions plus the European Manifestos Project's coding of postwar party platforms.[16] The other two measures assess the ideological range of governments along clerical–secular and regime support dimensions on the basis of Dodd's (1976) party scales. The question is, How well do these measures perform against the bargaining environment factors that constituted the core of the unified model, particularly when economic conditions are brought under consideration?

TABLE 2

Effects of Bargaining Complexity, Ideological Diversity, and the Economy on Government Survival

	MODEL					
COVARIATES	1	2	3	4	5	6
Majority status	−.69	−.65	−1.76	−1.43	−1.61	−1.90
	(.15)	(.21)	(.28)	(.32)	(.32)	(.29)
Investiture	.54	.59	—	.54	.60	.73
	(.15)	(.22)		(.22)	(.22)	(.21)
Postelection	−.66	−.49	—	−.36	−.30	−.36
	(.15)	(.20)		(.21)	(.21)	(.21)
Caretaker status	.54	.91	—	—	—	—
	(.47)	(.56)				
Formation attempts	.11	.15	—	.11	—	—
	(.05)	(.06)		(.06)		
Effective parties	.09	.07	—	—	—	—
	(.05)	(.08)				
Polarization	4.15	4.08	—	2.32	1.42	—
	(.64)	(.77)		(.96)	(1.06)	
Ideological diversity	—	—	—	.42	.54	—
				(.18)	(.18)	
Left/right	—	—	.33	—	—	.41
			(.11)			(.11)
Clerical/secular	—	—	.29	—	—	.19
			(.07)			(.07)
Antisystem/prosystem	—	—	.29	—	—	.16
			(.07)			(.07)
Returnability	—	—	—	.99	1.28	1.46
				(.52)	(.52)	(.50)
Inflation rate	—	—	—	—	.018	.038
					(.021)	(.020)
Change in unemp. rate	—	—	—	—	.286	.283
					(.070)	(.070)
Log-likelihood	−1120.5	−536.7	−548.8	−533.1	−531.2	−530.7
N	359	207	208	207	208	208

Note: Entries are partial likelihood coefficients. Standard errors are in parentheses.

The analysis of the interplay of these factors begins with a reestimation of the unified model on the present data set, using the PL method. The results are given in Table 2, Model 1, which follows Laver and Schofield's (1990) lead in measuring party system fragmentation with the more easily interpreted "effective number of parties" index (Laakso and Taagepera 1979), rather than with the fractionalization measure. In other respects, the covariate specification is the same. Given that the two models differ in all other significant features (the type of model, the countries sampled, the definition of key concepts), Model 1 mirrors the results reported by King and his colleagues remarkably well. Among bargaining environment indicators, the number of formation attempts shows about the same effect on government survival as in the earlier model and the polarization effect has become stronger. In light of the preceding discussion, it may seem surprising that the effective number of parties is not significantly related to government survival at the .05 level, but this is also true of the fractionalization measure in the unified model.

We have seen that the inflation and unemployment indicators are not available for all countries and years covered in the sample; the same holds for the European Manifestos Project data.[17] Accordingly, Model 2 reassesses the unified model on the smaller set of cases for which complete data can be obtained. Although coefficient values are affected to some extent by the reduction in cases, the key finding is that all covariates significantly related to government survival in Model 1 remain significant in Model 2. This result greatly facilitates the task of evaluating the two rival hypotheses.

The second of these hypotheses is given a preliminary test in Model 3, using the same subset of cases; the test shows that all three ideological diversity measures described earlier (together with majority status) are related to government survival significantly and in the expected direction.[18] The full effect of the concept will be assessed in subsequent models by summing these measures to form an "ideological diversity index," using the coefficients listed in Model 3 as weights. When substituted for its component measures in Model 3, this index receives a coefficient of 1.00.[19] What happens to this coefficient when the bargaining environment indicators are brought into the picture is, of course, another matter.

Before turning to that issue, there is a second aspect of the ideological diversity interpretation to be considered. To the extent that the antisystem presence in a legislature reduces the range of possible governmental coalitions, the probability that any member of an outgoing government will participate in the government that replaces it is likely to increase. Where the antisystem presence is substantial, therefore, parties may be tempted to defect from an uncomfortably diverse coalition government in the expectation that they will probably have to be included in the government that replaces it. This propensity can be assessed for each system by determining its "returnability," that is, the proportion of government parties that return to power in the next government following a collapse or early termination.[20]

The two covariates reflective of the ideological diversity hypothesis—the ideological diversity index and returnability—are tested against the significant covariates of the unified model in Model 4. The results reported in this model indicate that ideological diversity and returnability do substantially reduce the roles of the bargaining environment indicators, with only polarization continuing to show a significant effect. The coefficient associated with the ideological diversity index, although still significant, is also lowered very noticeably from 1.00 to .42—a reflection of the variance it shares with polarization. Model 4 thus shows the extent to which the two sets of covariates overlap in their capacity to account for government survival but fails to eliminate either set decisively. The next step is to determine whether the two economic covariates—the inflation rate and the change in the unemployment rate—can achieve this end.

This issue is addressed in Model 5, which adds the inflation rate and the change in the unemployment rate to the significant covariates of Model 4.[21] The principal result engendered by the inclusion of the economic covariates is the reduction in the coefficient associated with polarization, which is now statistically insignificant. It is also notable that the effect produced by the inflation rate is also quite insignificant. These findings suggest that the two covariates overlap to some extent in their effects on government survival—in other words, that systems with a large antisystem presence also tend to have higher inflation rates and that both features correlate with government survival. The implication is that one of these covariates must be removed from the model specification.

From a theoretical standpoint, it is clear that polarization is the one to be discarded. As I have noted, polarization is a poor indicator of bargaining complexity because it registers the narrowing of coalitional alternatives conveyed by the presence of noncoalitionable parties—precisely the reverse of what was intended.[22] In terms of the ideological diversity hypothesis, polarization functions not as a direct cause but, rather, as a surrogate measure, standing in for the ideological diversity within governments and other features that lead to government instability. The results in Model 5 suggest what those other features are: the incentive to defect from governments engendered by high returnability rates and the higher inflation experienced by polarized systems in the present sample.

The effect of removing polarization from the specification is given in the final model of Table 2. The three ideological diversity measures are entered separately in Model 6 in order to demonstrate that each is related significantly to government survival. The model also shows a noticeably larger coefficient associated with the change in the unemployment rate than was evident previously (see Table 1), indicating that controlling for other relevant factors actually enhances the role of that factor. As for inflation, its coefficient has nearly tripled, although its statistical significance is not particularly high.

The PL analyses presented in Table 2 demonstrate that the two economic covariates identified earlier not only contribute to the statistical explanation of government survival but also decidedly tilt the balance in the contest between its two main theoretical interpretations. This conclusion, moreover, is not reached at a price in terms of statistical power: like King and his colleagues' model, Model 6 is capable of accounting for all intercountry differences in termination rates (see Appendix B for details). Nor is it dependent upon the inclusion of support parties in the definition of governments: the model emerges intact even when governments are defined to include cabinet members only; cabinet members plus formally allied parties; or cabinet members plus formal, declared, and undeclared supporting parties. Finally, it does not imply any dependency between the presence of ideological diversity and the impact of the economic covariates. In fact, tests performed to detect an interaction effect in Model 6 failed to turn up any significant tendency for economic conditions to have a greater effect at higher levels of ideological diversity.[23] Economic factors, it appears, affect homogeneous and diverse governments alike.

IDEOLOGIES AND ERAS

Although the role played by economic factors in government survival appears to be independent of the level of ideological diversity within governments, it need not follow that it is unrelated to their ideological *orientations*. It is a commonplace among political observers that left-wing governments concern themselves more with unemployment than inflation and that right-wing governments reverse that order of priorities. These preferences are often linked to the perceived economic interests of their supporters. High unemployment tends to be seen on the Left as a "human" cost of capitalism that disproportionately affects the worse-off and requires government intervention to rectify or at least alleviate; concerns expressed over inflation may appear, however, as a pretext for beating back union wage demands. The Right takes a less zero-sum view of the workings of capitalism; but it does find economic virtue in the creation and maintenance of a favorable business climate, which typically involves price stability as well as moderation in tax and social spending levels. Thus, the expectation has been, at least until the rightward drift in economic policy of the 1980s, that left-wing governments will cater to their working-class base by pursuing high-employment policies even if it stimulates inflation, whereas right-wing governments will focus on their middle-class constituency's concern for price stability even if it risks jobs.

To what extent are these expectations justified? Table 3 addresses this question by presenting the records of socialist, bourgeois, and mixed governments on the two economic indicators most closely related to government survival: the inflation rate and the change in the unemployment rate. The three government types were created by defining a left–right ordering of parties for each system; dividing these orderings into socialist and nonsocialist components; and coding each government as composed wholly from the socialist component, wholly from the bourgeois component, or containing parties from both components. To assess the performance of the three types of government, mean values of the economic covariates were estimated for each government from interpolated daily values.[24] These means were then averaged across governments of each type.

Table 3 does, in fact, reveal broad tendencies that conform to my outlined expectations. Although inflation and changes in unemployment show considerable fluctuation over the lifespans of all types of governments (as indicated by the standard deviations), socialist governments clearly do experience higher mean levels of inflation than bourgeois governments. Conversely, bourgeois governments typically experience a greater tendency for unemployment rates to increase than do socialist governments. Mixed governments appear to represent the worst of both worlds: inflation rates rivaling those of socialist governments and unemployment growth matching that of bourgeois governments. Interestingly, these patterns hold up even for the final month of existence for each type of government, as the final row for each covariate reveals. This finding suggests that bourgeois governments do not fall when they lose their grip on inflation, nor socialist governments when unemployment escapes their control. Instead, both appear to terminate despite their continuing strong records in these domains.

This conclusion is reinforced by the results obtained when the full model is estimated separately for the three types of government. The estimated coefficients for the economic covariates in these analyses are given in Table 4. Models 7–9 present a pattern of findings that at first glance may appear surprising: changes in unemployment are significantly related to government survival among bourgeois and mixed governments only (Models 7–8), while inflation shows a significant effect solely among socialist governments (Model 9). These relationships cannot be due to the tendency for bourgeois governments to outperform socialist ones on inflation or to be outperformed by them on unemployment, because the models relate each government's economic record only to those of other governments of the same type. Thus, Model 7 associates higher termination rates with bourgeois governments that undergo greater increases in unemployment relative to other bourgeois governments—and the same goes for socialist governments with respect to inflation (Model 9). The most likely interpretation of these results is that bourgeois governments and socialist governments are perceived as being uniquely equipped to handle inflation and unemployment, respectively (a perception borne out by the evidence in Table 3) and are therefore vulnerable only to adverse developments in the other indicator. The finding that mixed governments are "punished" only for unemployment despite their poor records in both areas underscores the dominance of unemployment in the postwar era. It may be that inflation plays a significant role in socialist government terminations only because the socialists' credibility on unemployment is so high.[25]

TABLE 3

Inflation and Unemployment in Different Types of Governments

ECONOMIC INDICATOR	TYPE OF GOVERNMENT		
	BOURGEOIS	MIXED	SOCIALIST
Inflation			
Average monthly value	5.37	7.29	7.18
	(95)	(92)	(40)
SD in monthly values	.95	.91	1.07
	(89)	(89)	(40)
Final month value	5.37	7.18	7.39
	(95)	(92)	(42)
Change in Unemployment			
Average monthly value	.24	.24	.03
	(95)	(92)	(40)
SD in monthly values	.54	.36	.44
	(89)	(89)	(40)
Final month value	.34	.22	−.02
	(95)	(92)	(40)

Note: Main entries are mean values for each type of government, with the number of governments given in parentheses. Only governments with valid data for both economic covariates are included. Values for individual governments were derived by dividing government durations into 30-day intervals ("months") and estimating the annual rates of inflation and change in unemployment at the beginning of each interval (using December figures). Means and standard deviations were then calculated over the intervals and the values in the final interval noted.

A second dimension along which the effects of the economic factors on government survival may vary relates to the period during which governments held office. Few would dispute that the oil crisis of 1973 marked a critical watershed in Western economic history, the end of the long postwar economic boom. Certainly, both inflation and unemployment became much worse in the 1970s and 1980s, as Figure 1 indicates. This raises the question whether economic conditions have come to assume a more pronounced

TABLE 4

Economic Influences on Government Survival, by Type of Government

	MODEL (TYPE OF GOVERNMENT)		
COVARIATES	7 (BOURGEOIS)	8 (MIXED)	9 (SOCIALIST)
Inflation rate	.001	.038	.178
	(.036)	(.031)	(.060)
Change in unemp. rate over prec. year	.248	.760	−.101
	(.098)	(.167)	(.245)
N	91	78	41

Note: Entries are partial likelihood coefficients derived from reestimations of the full model (Model 6) for the appropriate subset of cases. Standard errors are given in parentheses.

role in government survival in the period since October 1973.

This issue is explored in Table 5, which shows the effects of the economic covariates when the full model is estimated separately for the two eras defined by the 1973 oil embargo. A comparison of Models 10 and 11 provides scant support for the hypothesis of a general increase in the impact of the economy on government survival: while the impact of changes in unemployment is much stronger after 1973, that of inflation shows a comparable decrease. This shift in emphasis, moreover, is not engendered by any increase in the proportion of bourgeois governments in the latter period.

The analyses reported in Table 5 indicate that the principal consequence of the altered economic conditions of the postboom period has been to bring the causal role of changes in unemployment to the fore. The relatively weak role played by inflation in the overall model (Model 6) may be attributed to this fact. This is not to say that inflation is irrelevant to government survival. If the analysis is confined to the 1960s and 1970s, when consumer prices showed the greater change, inflation displays a much more significant impact on survival than changes in unemployment.[26] But the stronger role in the postboom era clearly belongs to the rising rates of unemployment, the roller coaster ride in inflation rates notwithstanding.

TABLE 5

Economic Influences on Government Survival before and after 1973

	MODEL	
COVARIATES	10 (TERMINATIONS BEFORE NOVEMBER 1973)	11 (TERMINATIONS AFTER OCTOBER 1973)
Inflation rate	.146	.039
	(.072)	(.029)
Change in unemp. rate from prec. year	.217	.453
	(.093)	(.116)
N	105	103

Note: Entries are partial likelihood coefficients derived from reestimations of the full model (Model 6) for the appropriate period. Standard errors are given in parentheses.

DISCUSSION

A conceptualization of outside events and their impact on government survival very different from the events theorists' has guided my research. For one thing, the events under scrutiny are not the idiosyncratic occurrences—the forced landing of a terrorist controlled aircraft or the assassination of a prime minister—that occasionally topple governments but the more systematic and more fundamental economic trends that form a large part of their day-to-day concerns. For another, the interconnection between these trends and government survival is viewed as reciprocal, rather than one-way: governments affect economic conditions, as well as being affected by them.[27]

The influence governments have on their economies can be seen quite clearly in the pattern of relationships linking ideological orientation with success in containing inflation and unemployment. The evidence indicates that bourgeois governments, as expected, do experience lower inflation rates than socialist governments. Conversely, unemployment rates are relatively less likely to rise during socialist administrations. These associations do not establish a causal role for government policy, to be sure; but it is difficult to interpret them in any other terms. Less expected are the findings concerning the reverse link, which suggest that governments are vulnerable only on the economic indicator they are generally less adept at handling. Thus, socialist governments do not terminate when their unemployment records fall behind the norm for governments of that type (presumably because their bourgeois opponents do not provide a credible alternative on the issue); but they are at greater risk if their inflation performance is off the mark. For bourgeois and mixed governments, the immunity pertains to inflation and the vulnerability to unemployment.

The inflation rate and the change in the unemployment rate turn out not only to affect government survival but also to play a key role in the debate pitting bargaining environment and ideological diversity interpretations of the phenomenon against each other. Although the introduction of the ideological diversity within governments seriously undermines the causal role attributed to bargaining environment complexity, the economic covariates dispense a further blow by eliminating the significant effect of its one remaining indicator, the legislature's polarization, or antisystem party presence. In the ideological diversity interpretation, a strong representation for antisystem parties in a legislature inhibits government survival both by necessitating the formation of ideologically diverse governments from among the prosystem parties and by increasing the likelihood that parties that defect from one government will have to be included in its successor. Model 6 provides evidence for both effects. In contrast, none of the three proposed bargaining environment measures is able to enter the model significantly; nor do they, taken individually or as a group, significantly improve its log-likelihood.

What perspective on government survival is suggested by the findings of this study? Perhaps the most basic feature of government survival uncovered here is the extent to which its immediate sources lie not with the larger parliamentary environment or with the outside world but with the governments themselves. This is indicated both by the importance of government characteristics such as ideological diversity and majority status as well as by the capacity governments have to influence the economic indicators that matter most for survival. The larger political and economic environments are not banished from the picture, however. Not only do parliamentary settings impose constraints on the composition of governments and global economic trends largely escaping their control, but the exact timing by which governments terminate under the pressure of ideological differences must depend, in part, on circumstances of an idiosyncratic nature. Whatever these circumstances may be, a heightened vulnerability to economic trends appears not to be involved. On the contrary, governments of all types, including the minimally diverse single-party governments, are at risk to adverse economic developments.

The mechanism by which these developments undermine government survival remains largely a matter of speculation at this point. Some idea of what might be involved can be gleaned, however, from the economic records associated with different types of termination. In particular, it is notable that governments that were defeated in parliament or resigned before the end of their mandate (excluding those that terminated in order to add a coalition member) had significantly higher mean levels of the two economic covariates than did other governments.[28] This suggests that impatience with government economic strategy or a desire not to be associated with economic failure encourages parties to defect from governing coalitions. A firmer verdict must await more detailed investigations.

Concerning the future, my findings contain both favorable and unfavorable signs for government stability in West Europe. On the favorable side is the finding that survival in office depends on changes in unemployment rates rather than on their actual levels. This result indicates that the higher overall levels of unemployment of the last 15 years may not be in themselves inimical to stability. Nevertheless, in view of the powerful impact of that factor (each 1% rise boosts the termination rate by about one-third in Model 6), any surge in unemployment consequent upon the restructuring of the European economy after 1992 could significantly undermine government durability. The days when the very survival of parliamentary regimes in West Europe was at issue may be long past, but the potential for future governmental instability should not be discounted.

APPENDIX A. DATA SOURCES FOR THE ECONOMIC COVARIATES

The December inflation values for the periods 1946–63, 1964–69, and 1970–89 are derived from consumer price data listed in the International Labor Organization (ILO) *International Labor Review* (1945–63) and *Bulletin of Labor Statistics* (1964–70) and the International Monetary Fund (IMF) *International Financial Statistics* (1970–90), respectively. For years in which only consumer price index values are given, the inflation rate was calculated from annual percentage changes in these values. Following the IMF practise, these rates are calculated only to the nearest whole number whenever the index values themselves are given in whole numbers. Annual values were taken from the IMF *International Financial Statistics Yearbook* (1979, 1983, 1989).

The primary source for the unemployment data is the *UN Monthly Bulletin of Statistics* (1947–90). For earlier years, this source was supplemented, where appropriate, by data from the *International Labor Review*. Because of gaps and inconsistencies in the UN data for Spain and the Netherlands (in the 1980s), data from the Organization for Economic Cooperation and Development (OECD) *Main Economic Indicators: Historical Statistics 1969–88* was substituted in those cases. An "earlier series" category is established whenever a serious discontinuity occurs in rates owing to changes in how they were calculated.

The gross domestic product figures are from the *OECD Economic Outlook* for the period from 1970 and the *OECD National Accounts*, vol. 1, *Main Aggregates, 1950–78* and *Main Aggregates, 1960–88* for the earlier years. An IMF version of this indicator was also coded, based upon the series reported in the *International Financial Statistics Yearbook* (1983–89); but it did not yield substantially different results and is not discussed in this study.

APPENDIX B. MODEL TESTS FOR INTERCOUNTRY DIFFERENCES

The tests involve determining whether the log likelihood improves significantly when a set of dummy variables representing individual countries is added to Model 6. If it does, the implication is that the original covariates are unable to account for all significant intercountry differences in survival rates. Because returnability is constant for individual systems, it is necessary to exclude two country dummies to avoid collinearity. Norway and Sweden were chosen because of their similarities in mean duration and other factors. For cabinet parties only, cabinet plus formally allied parties, and cabinet plus all supporting parties (three of the four definitions of a government) the increases in the log likelihoods generated by the addition of the country dummies are insignificant at the .05 level. (The χ^2 values for these increases are 12.3, 14.7, and 12.0, respectively, with $\chi^2 \geq 16.9$ required for significance at nine degrees of freedom.) The exception, the definition that includes support parties only when their support is openly declared ($\chi^2 = 18.9$), depends on one unusual circumstance: the obligation to treat the British Labour government of 1976–79 as three distinct governments because the Liberals agreed to support it for part of that period. The effect is to make Britain appear less stable than its covariate values and the model would indicate.

The country dummies can also be used to provide a test of the appropriateness of replacing polarization with ideological diversity. The test consists of adding the country dummies to Model 5, which includes both ideological diversity and polarization. The result is that the coefficients for both covariates increase modestly. However, the standard error for polarization increases very drastically; and the coefficient becomes even more insignificant. The t-value for ideological diversity, conversely, is not affected and the covariate remains highly significant. This suggests that polarization's role in government survival, unlike that of ideological diversity, may be an artifact of its high concentration in one or more countries.

Notes

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1. The determination of an appropriate distribution for government duration data (which is equivalent to determining the nature of the underlying trend in government survival net of all measured causal factors) is taken up in Warwick 1992(b), a companion piece to the present paper.

2. Elsewhere, Robertson (1983a) does relate changes in the rate of government collapse in various countries to changes in their unemployment and inflation rates. Individual governments, however, do not constitute the unit of analysis.

3. The log-linear specification is not strictly necessary but has the advantage of precluding negative predicted hazard rates. (A rate of termination cannot be less than zero.)

4. The partial likelihood (PL) method is introduced in Allison 1984 and developed in more detail in Tuma and Hannan 1984 and Blossfeld, Hamerle, and Mayer 1989. The technique does require the assumption that the hazard rates of any two cases be proportional over time; but further testing indicates that this assumption is met in these data (Warwick 1992[b]). It also assumes that any random errors are identical across cases, so that differences among cases are accounted for solely by differences in their covariate values (Tuma and Hannan 1984, 253–54). This assumption is normally violated in practise, but apparently without serious consequences.

5. If X_1 and X_2 are the values of covariate X at times t_1 and t_2, respectively, the interpolated value of X at a time T that falls between them would be

$$X_T = X_1 + \{(T - t_1)(X_2 - X_1)\}/(t_2 - t_1),$$

where all times are measured in days from some fixed date, such as the date the government was formed. The BMDP program 2L, which provides a highly convenient facility for interpolating time-dependent covariate values, was used to estimate all the PL models I report.

6. This is achieved statistically by estimating the contribution of these cases to the overall likelihood by means of the survivor function, which represents the probability of surviving to or beyond any given duration value, rather than with the density function.

7. This follows the practice of Browne, Gleiber, and Mashoba (1984) and King and his colleagues (1990), except in the case of resignations that are not accepted by a head of state. Such resignations are regarded as terminations in this study (although not in the others).

8. This assumption was checked by comparing the relationships between the termination rate and the inflation and unemployment indicators based on annual data with those produced using daily interpolations from December data (to be discussed). The December-based versions generally yielded the stronger relationships.

9. The simple (bivariate) PL coefficient for the actual monthly inflation rate is .056 (t = 3.29), while that for the pseudo-monthly rate is .064 (t = 3.56). This pattern holds up when the other covariates (to be discussed) are included in the analysis. In these tests, the actual monthly data attribute a given monthly rate to each day of that month. The interpolated data attribute the estimated value for the first day of a month to each day of that month. Because of the enormity of the coding task this analysis entails, it was performed only on the 208 cases for which complete data are available. The test is inappropriate for unemployment because actual monthly unemployment rates are contaminated by seasonal effects.

10. They are Austria, Belgium, Denmark, Finland, France (the Fourth Republic), Iceland, Ireland, Italy, Luxembourg, the Netherlands, Norway, Portugal (from 1976), Spain (from 1979), Sweden, the United Kingdom, and West Germany.

11. The relative lack of precision in consumer price index numbers before 1964 is translated here into a lesser degree of precision in inflation rates, which are calculated only to the nearest whole number for that period. A comparison of results produced with and without the pre-1964 data indicates that the lack of precision of the earlier data affects relationships with government survival only slightly.

12. This could happen, for example, if economic downturns tend to produce majority governments. Since majority governments typically last longer than minority ones, unfavorable economic indicators might appear (spuriously) to be associated with longevity in office. It should also be noted that other factors may also be affecting the relationships reported in the table. Although uncontrolled here, they are taken into account in the subsequent analyses.

13. Using covariates based only upon the most recent unemployment series does not change these results very much: the PL coefficients for the change, proportional change, and absolute change versions are .309 (t = 3.66), .277 (t = 1.29), and .130 (t = 1.38), respectively. In addition, subsequent analyses based on these versions yield the same basic findings as those I shall report for the "all series" versions.

14. The same conclusion holds for the growth in gross domestic product: further testing indicates that its effect on the rate of termination is almost entirely due to its association with inflation and unemployment.

15. The criterion is not foolproof because any of the relationships reported in Table 1 could be suppressed by other factors. For this reason, the final model (6) developed in the next section was reestimated using the various alternative indicators of the economic covariates in the model. In all cases, the signs of the estimated coefficients were the same—and the significance levels lower—when the alternatives were employed.

16. The three sets of scales are Dodd's (1976) economic conflict scales (five-point version); Browne, Gleiber, and Mashoba's (1984) refinement of the Dodd scales; and socialist-bourgeois dichotomous scales based upon my earlier work (Warwick 1979). Each set of scales was subjected to a discriminant analysis using the European Manifestos Project data (Budge, Robertson, and Hearl 1986). The new, or "predicted," scale positions produced from these analyses, together with the first principal component of the European Manifestos Project data, formed the basis for four assessments of the left–right range of each government. Because these assessments intercorrelate so highly, they were subjected to another principal components analysis. The first principal component from this analysis, which accounts for over 95% of their common variance, constitutes the final measure of left–right diversity. Various other formulations, including those based on other source scales and those based on standard deviations rather than ranges, were also tested; this one proved to be the most successful.

17. The European Manifestos Project data exclude Finland, Iceland, Portugal, and Spain. Given the limited period of democratic rule in Portugal and Spain and the gaps in the unemployment data for Iceland and Finland, the number of cases lost through the use of European Manifestos Project data amounts to just 20.

18. This is also true when the entire set of cases for which these measures are available (n = 284) is used.

19. Although other methods of index construction could be utilized, this method was chosen because it leaves all other estimated parameters of the model unaltered.

20. Each system receives a returnability value based on its

record over the entire postwar period. It is assumed that this value reflects an underlying likelihood that is known to parliamentary actors throughout the period.

21. The postelection covariate falls somewhat below significance at the .05 level in Model 4 but was left in the analysis because its weaker role is partly due to the narrowing of the sample, as a comparison of Models 1 and 2 reveals. Had the reduced sample been more representative of the entire data set, it is a reasonable assumption that this covariate would have displayed a stronger and more significant effect.

22. This conclusion is supported by a correlation of .366 ($p < .01$) between polarization and returnability, indicating that the chances for a government party to return to power following an early government termination increase with the size of the antisystem presence in the legislature.

23. The tests consisted of adding interaction terms composed of the product of each economic covariate and the ideological diversity index to the model. Neither of these interaction terms, whether entered singly or together, proved to be significantly related to the termination rate.

24. The ideological orderings were based largely on Castles and Mair's (1984) left–right political scales, supplemented by the Browne, Gleiber, and Mashoba (1984) socioeconomic scales and by country-specific sources where appropriate. Mean economic indicator values for each government were derived by dividing the government's lifespan into intervals of 30 days, calculating the interpolated value at the start of each interval, and averaging these values over the intervals of the government's life. Thirty-day intervals were used in order to facilitate computations and to approximate the monthly intervals for which economic data are usually reported.

25. The patterns shown in Table 4 hold up when only majority governments (to control for the possibility that homogeneous minority governments may actually be dependent on parties of a different ideological orientation) and when only coalition governments are examined.

26. A reestimation of Model 6 for the 1960–79 period ($n = 91$) results in a PL coefficient of .087 ($t = 3.17$) for inflation and .177 ($t = 1.93$) for changes in unemployment.

27. This does not make the economic covariates endogeneous in the full model (Model 6). While their effects on survival do depend on the government's ideological orientation, they are independent of ideological diversity or any of the other covariates entered in the model. (Ideological orientation itself is not a direct cause of survival.) This is indicated by the finding that the introduction of the economic covariates into a model containing only the fixed covariates scarcely changes the coefficients associated with the latter. Thus, the economic covariates occur later in the temporal ordering than the fixed covariates; but they do not play the role of intervening variables.

28. Of the 208 cases utilized in the full model, the 95 that terminated in these circumstances had mean rates of inflation and change in unemployment of 7.3% and .4%, respectively. The corresponding values for the other cases are 5.9% and .0%. T-tests indicate that the differences between the two groups are significant at the .01 level in both instances.

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POLI: AN EXPERT SYSTEM MODEL OF U.S. FOREIGN POLICY BELIEF SYSTEMS

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One of the more fruitful systematic approaches to the study of foreign policy views nations as information processors and explains foreign policy as a function of information and processing thereon. Much of such processing depends on the prior beliefs of decision makers, but for a variety of reasons these systems of prior beliefs are very difficult to analyze using "standard" empirical and modeling techniques. Computational modeling, including expert system technology, provides an alternative methodology that allows such analysis. I present Policy Arguer (POLI), an expert system model of U.S. foreign policy making in Asia. POLI has been very successful in reproducing actual U.S. responses to events in Asia from the 1950s. More important, POLI recreates policy debate in the form of the arguments and counterarguments used to justify policy alternatives. The model explains foreign policy outputs in terms of a complete process from foreign policy beliefs, through debate, to choice.

Foreign policy decision making has been subject to formal analysis only in recent decades. Traditionally, scholars have taken a historical approach to the study of foreign policy, emphasizing descriptive detail and realism. Their investigations, often in the form of case studies, attempt to capture the rich complexity of actors and relations among actors. Formal studies, on the other hand, use different rules of argument and evidence, sacrificing the descriptive detail of the historical approach for much greater explanatory power. They represent the world with idealized models designed to capture in tight focus the important actors and relations among actors.

Despite important differences between formal and historical approaches, they also have similarities. Each attempts to understand the same set of real world processes of foreign policy decision making. Each constructs a representation of these processes according to organizing principles (theory or pretheory). Neither simply describes the world "as it is."

The Policy Arguer Project exploits the similarities between formal and traditional studies to build a bridge between them. Using the computational power of modern computers, formal models can achieve much of the rich complexity of historical representations, while maintaining analytic focus. Computational modeling techniques allow the analyst to raise the level of realism of formal models of foreign policy decision making. In the Policy Arguer (POLI), I apply computational modeling techniques to describe and explain particular foreign policy choices in response to world events, using U.S. decisions on policies toward Asia as my research domain. I shall focus on POLI I, which represents the U.S. foreign-policy-making system of the 1950s.

COGNITIVE APPROACHES TO FOREIGN POLICY ANALYSIS

The intellectual heritage of the POLI Project can be traced to the considerable work, both formal and traditional, on the role of *beliefs* in foreign policy decision making. Snyder, Bruck, and Sapin (1962) introduced the systematic study of foreign policy belief systems as an integral part of the decision-making frame of reference. They advocated the analysis of national behavior as a product of the *subjective* perception and interpretation of a situation by national decision makers, their expectations from various courses of action, and their choice processes. To use Holsti's classic description, a belief system is "a set of lenses through which information concerning the physical and social environment is received. It orients the individual to his environment, defining it for him and identifying for him its salient characteristics. . . . [And it] has the function of the establishment of goals and the ordering of preferences" (1962, 245). Belief systems, as cognitive structures that mediate experience for individuals, are central to the descriptively detailed analysis of decision making.

A number of foreign policy analysts have emphasized the importance of the *images*, embedded in larger belief structures, that policy makers have about the world and about other actors (Boulding 1956, 1959; Glad 1983; Glad and Taber 1991; Holsti 1962, 1967; Jervis 1970, 1976; Tetlock 1983). This work has primarily focused on the pathological impact of images on cognitive processing, particularly on perception and diagnosis. Through selective perception and distortion, decision makers bias incoming information about the world to conform with preconception. In addition, decision makers may develop rigid belief systems containing highly simplified images that restrict flexibility in the diagnosis of world events and in the choice among perceived alternatives.

In contrast to the emphasis of most image research (that simplified belief structures degrade the quality of information processing), Nathan Leites (1951) and Alexander George (1969, 1979) focused on the necessity of relatively simple belief systems for cognitive functioning. They developed the *operational code* construct to describe these essential cognitive structures. Decision makers' operational codes consist of their fundamental beliefs about the dynamics of international relations and about the efficacy of human action, coupled with their simplifying images of salient international actors (as, e.g., friendly or unfriendly). These beliefs enable decision makers to process information and examine potential courses of action efficiently. Of course, a decision maker's operational code has a tremendous impact on the particular policies that will be advocated or opposed by that decision maker; and as such, it can be a useful analytic tool for the explanation of policy choice (Walker 1983).[1]

Belief structures of this sort may be formally modeled as *cognitive maps*, in which elements of belief and the relationships among these elements are graphically represented (Axelrod 1976). These structural representations of belief systems allow analysts to predict behavior from a decision maker's operational code. Unfortunately, unless they are highly simplified, cognitive maps quickly become too large and unwieldy to analyze using standard methods. Moreover, static representations of cognitive structures like operational codes or cognitive maps cannot deal effectively with important cognitive processes, including reasoning, counterarguing, and learning. For these and other reasons, the cognitive map approach has not been as productive as its early advocates anticipated. Indeed, many analysts of foreign policy decision making have moved away from static representations of cognitive structure toward models or descriptions of the *processes* involved (Bennett 1981; Cottam 1986; Hermann and Coate 1982; Holsti 1977; Janis and Mann 1977; Powell, Dyson, and Purkitt 1987; Purkitt and Dyson 1985).

COMPUTATIONAL MODELING IN INTERNATIONAL RELATIONS

For a formal modeler, the complexity inherent in the decision-making approach is daunting, requiring a modeling language capable of expressing its essential features.[2] Computational modeling, developed in *cognitive science* (roughly, the intersection of cognitive psychology, computer science, and the philosophy of mind) provides a set of tools designed for precisely this problem.[3] Unlike earlier formalisms, computational models can represent rich cognitive structures *and* the processes that link structure to decision. In the field of international politics, these techniques allow the formal analyst to return to the issues and theoretical approaches introduced by Snyder, Bruck, and Sapin (1962).

The application of computational modeling ideas in international relations has a sparse but surprisingly long history, beginning with work at the Massachusetts Institute of Technology in the late 1960s and through the 1970s (Alker, Bennett, and Mefford 1980; Alker and Christensen 1972; Alker and Greenberg 1971, 1976; Bennett and Alker 1977; Pool and Kessler 1969). Rather than attempting a comprehensive review (see Benfer, Brent, and Furbee 1991; Hudson 1991), I will briefly describe several projects in international relations that provided direct inspiration in the development of POLI.

Decision makers must generate meaningful representations of new information before that information can be used in reasoning, that is, they must interpret incoming information. Indeed, how they do this—and in particular the issue of perceptual bias in this process—is a major topic of belief system research, particularly for those who focus on images. Not surprisingly, it is also a central issue in most foreign policy computational modeling projects.

Building on the work of Schank and Abelson (1977), Carbonell (1978) developed a set of programs capable of quite complex interpretations of political events. The primary contribution of Carbonell's POLITICS for my purposes is the application of organized structures of prior beliefs in the interpretation of new events. In this approach, "understanding" is the process of relating external information to internally represented concepts and ideas. An object in the environment (say, North Korea) has meaning to a decision maker only if it can be mapped onto some set of internally represented (i.e., "known" or "believed") concepts (say, "communist" or "regional imperialist"). Decision makers with different internal belief representations will have different interpretations of an event involving North Korea. Although the particular cognitive organization used in POLITICS (ideologies, scripts, and context-dependent inference rules) differs somewhat from that used in POLI, the idea that complex representations of belief provide a fertile ground for event interpretation and the reasoning process is important in both models (see also Seitz 1987).

Thorson and Sylvan (1982) modeled the decision making of John F. Kennedy in the 1962 Cuban Missile Crisis. Kennedy's prior beliefs, on the basis of which he interprets and reacts to events during the crisis, are represented in JFK/CBA as a set of production rules, or IF→THEN statements. Though small by comparison with some other rule-based models, JFK/CBA contains a far more detailed theory of Kennedy's relevant beliefs than could be expressed intelligibly as a cognitive map. JFK/CBA recognizes 14 possible states of the world during the missile crisis, ranging from "Soviet missiles are removed from Cuba" to either the United States or the Soviet Union "initiates a war." U.S. and Soviet actions transform the current state into a new state. Kennedy's beliefs, embodied in the IF→THEN rules, are used to evaluate possible courses of action, as well as the plausibility of possible Soviet reactions, eventually leading to a decision and a state transformation. A run of the model, beginning with the discovery of missile base construction in Cuba, produces a simulated trace that can be compared to the well-documented historical case. In this basic validity test, JFK/CBA fared very well. The simulation runs of the 1962 crisis, based on the computational representation of Kennedy's belief system, are remarkably similar to the actual historical trace.

JESSE, a computational model of Japanese energy policy making (Sylvan, Goel, and Chandrasekaran 1990) explicitly integrates cognitive and political factors. How, within political constraints, does the decision-making system interpret a situation and select a stored plan of action? These tasks—classification and plan selection—are accomplished in JESSE using a sophisticated combination of knowledge frames and rules. On the basis of prior knowledge, JESSE asks the user a series of pointed questions whose answers allow it to interpret an event according to its classification scheme. JESSE then chooses a plan of action from its repertoire of stored plans. For example, JESSE classified the energy situation following the 1979 Iranian revolution as (in simplified form) a serious threat to the availability of low-cost imported energy for Japan. From a large set of stored plans, JESSE then selected a number of policies to deal with this energy threat, including stockpiling energy and reducing demand.

JESSE and JFK/CBA demonstrate the usefulness of reducing the domain of analysis (see also Job and Johnson 1991). Knowledge-based, or *expert*, systems—a subset of computational models—gain considerable power by focusing on specific domains of knowledge, or belief. They process information on the basis of prior knowledge or beliefs about some problem domain, rather than relying on more general knowledge.[4] As we have seen, JESSE is designed to solve problems of Japanese energy security. It does very well in that domain but would be lost if presented with a problem from U.S. security policy. Like JESSE and JFK/CBA, POLI relies on knowledge and beliefs about a constrained domain, namely U.S. foreign policy making toward Asia. Following convention, I will call the specialized knowledge and beliefs of U.S. policymakers *expertise*—though this term does not necessarily connote accuracy or truth.

Anyone who takes seriously the role of prior beliefs in information processing must consider the question, Whence come prior beliefs? Thorsen and Sylvan (1982) and Carbonell (1978) emphasize the importance of *precedent* in the interpretation of new events (see also Alker and Christensen 1972; Bennett and Alker 1977; Tanaka 1984). Any decision maker's belief system will contain beliefs about "how the world works" in certain contexts; that is, beliefs about how the world worked in similar situations in the past. Considerable evidence from cognitive science suggests that analogies drawn from experience are key components in the interpretation process (Mefford 1985, 1987; Schrodt 1985). Case-based decision models rely heavily (often exclusively) on these stored examples for interpretation of and reasoning about new events. While POLI does not rely exclusively on historical analogies, some of its understanding of events does build on historical cases. For example, one interpretation of the 1950 North Korean invasion of South Korea, among several offered by POLI, rests on a perceived similarity to the behavior of Hitler after the 1938 Munich Conference. In this interpretation, the Korean invasion is understood by analogy with the 1938 case.

THE POLICY ARGUER

POLI is an expert system model of U.S. foreign policy decision making in which several competing views of the world drive the reasoning process.[5] POLI takes events, in the form "actor, verb, target, context" as input and recommends U.S. responses. For example, POLI I recommends 19 U.S. policies for the 1958 Taiwan Straits Crisis, including a direct military threat against China, an upgrade in military readiness, and a protest in the UN Security Council.

Following a typical expert system design, POLI has four components:

1. The *knowledge base* is POLI's representation of U.S. beliefs about policy toward Asia. It is composed of a set of paradigms, or shared belief systems, through time.
2. The *inference engine* controls the processing of information. In POLI, this is a forward chainer using fuzzy inferencing.[6]
3. *Working memory* is the space in which the inference engine compares input with the knowledge base, selects and uses pieces of knowledge to operate on the input, and produces output.
4. The *user interface* is the means of communication between the analyst and the model.

The key tasks in developing an expert system are the extraction and representation of the expert knowledge base that the model will use and the development of the inference engine, which controls how the expert knowledge is used.

Building POLI's Knowledge Base

Unlike experts in many other domains, U.S. foreign policy makers do not draw on a clear, consensual body of expert knowledge. Even in the 1950s, at the height of the Cold War, there was more than one way of thinking about the world. In general, multiple agents of different worldviews compete to affect policy outputs. These competing impulses arise from a set of quite different representations of the world. In fact, agents of different worldviews reason differently about the same input information and come to different conclusions.

For example, in the late 1940s there was a difference of opinion in the American policy making system about policy toward China. Key beliefs of different worldviews led to quite different conclusions:

1. Communism is not an implacable enemy but only a temporary one due to mistaken U.S. policies (implying that the United States should change its policy so as not to tread on communist toes, including in China).
2. Soviet communism is fundamentally opposed to U.S. interests, but other brands of communism might not be (implying that the United States should encourage Titoism in emerging communist movements and not automatically oppose all communist movements, including Mao's agrarian movement in China).
3. Communism is monolithic and opposed to U.S. interests, but the communist menace is centered on Europe; if it is contained in Europe, communism will fall of its own dead weight (implying European containment, excluding China).
4. Communism is monolithic and opposed to U.S. interests, and its immediate goals are worldwide; the failure to contain communism anywhere will weaken resistance everywhere (implying global containment including in China).

POLI represents this type of situation with multiple competing worldviews.

Although U.S. foreign policy making is not consensual, neither is it completely idiosyncratic. Patterns, or clusters, of shared beliefs emerge in the system and gain varying degrees of acceptance among individual policymakers. Of course, at extreme discrimination these belief clusters dissolve into distinct idiosyncratic worldviews. But I do not take this approach here. Rather, I work at a level between the individual and the organization: the level of shared belief systems, or *paradigms*.

This project assumes that a limited number of paradigms drive U.S. foreign policy and that they provide a useful organizing device. In this view, the individual proponents of a particular shared belief system are less important than the position of their paradigm in the ongoing foreign policy debate.[7] Though the individuals make the decisions, they do so as agents of one or another paradigm. POLI is similar in this respect to Carbonell's (1978) POLITICS, in which ideologies and other shared belief structures govern the interpretation of political conflicts.

Focusing on the information-processing system at the level of competing paradigms avoids several problems associated with similar work at the individual level of analysis. Most important, since individual policymakers are often strategic political actors, their expressed beliefs change across audiences and political fortunes. Surely such "beliefs" are poor predictors of their actual positions.

This would be a particularly grave problem for the Policy Arguer Project, since POLI is largely built on the public statements of politicians. However, these statements are not used as indicators of an individual's beliefs; rather, I use them as indicators of important beliefs in the system of ongoing policy argument. Public statements are important in their own right, even if they are not sincere representations of the speaker's own view. If they are persuasive, public statements reflect important beliefs in the political system. Indeed, to the extent that leaders feel compelled to express disingenuous beliefs, they are responding to the pressure of strong paradigms in the political culture. Individuals who share important beliefs from the dominant paradigm will push the argument; opponents may be forced to go along. In this perspective, public statements are important indicators of the paradigms that are engaged in policy arguments, even though they may not represent the speaker's "true" beliefs.

Second, some individual-level projects in foreign policy decision making focus too heavily on one important leader. Except in relatively rare crisis situations, the decisions of one leader do not control U.S. policy. A model of U.S. foreign policy making needs to account for the disputes in the system, for the policy arguments. POLI focuses on these policy arguments, both between opposed paradigms and within a single paradigm. Indeed, the treatment of uncertainty and belief in POLI enables me to trace the amount of support within a paradigm and within the system for any given line of argument.

Data Collection. The paradigms used in POLI I are extracted from the *Congressional Record* for 1949–60, through a type of content analysis. The procedure was to (1) identify a set of potential paradigms described in several U.S. foreign policy texts (George and Smoke 1974; Harrison 1978; Jervis 1976; LaFeber 1972; Nathan and Oliver 1985); (2) distinguish a preliminary set of core beliefs for each paradigm, again using the texts; (3) do a pilot study in the *Congressional Record* for a two-year period; (4) modify the set of paradigms and their core propositions according to the findings of the pilot study; and (5) use the modified paradigm set as the basis for a complete content analysis through the decade.

The first four steps in this process identified the bare skeletons of three paradigms in the early 1950s: militant anticommunism (MAC), pragmatic anticommunism (PAC), and isolationism (ISO).[8] A coding plan for the content analysis through the decade was also developed on the basis of this experience. Briefly, this plan entails six steps:

5a. Identify all references in the *Congressional Record* index under the headings "Asia" and "the Far East."
5b. Find and classify each entry by type and date. They may be speeches, insertions of articles, or floor debates.
5c. Read each entry and convert the logic used onto a code sheet. A completed code sheet is essentially a propositional inventory for the entry.
5d. Determine whether each entry conforms to the core propositions of one of the paradigms and classify the entry accordingly. In the 1950s, the

current three-paradigm scheme was sufficient to classify all the entries.

5e. Organize the logic found in the entries for each paradigm according to the structure I shall describe.

5f. Assign a certainty factor (CF), ranging from 0 (indeterminate) to 1.0 (true), to each datum in each paradigm and an overall CF to each paradigm. These CFs, based on the relative frequency of each proposition in the *Congressional Record*, indicate the level of confidence POLI will have in the truth value of the propositions. The certainty factor for a proposition is equal to the proportion of entries that invoked it in the given paradigm and time period. Core rules have the highest CFs because they represent common lines of reasoning in the paradigm. Peripheral propositions are invoked less frequently, are controversial, and have lower CFs. For example, the proposition that the United States should use atomic force in response to an indirect threat (through the domino principle) appears in three of the 37 MAC entries (speeches, articles, or debates) in the 1949–52 period; its CF = 3/37 or .08. Similarly, the CFs for the paradigms, which represent their relative dominance in the decision-making system for the time period, are based on simple frequency counts. For each time period, the proportion of entries supporting a paradigm is assigned as its default CF.

Following this plan, the paradigms for the 1950s, divided into three time periods (1949–52, 1953–56, and 1957–60), were compiled. Each paradigm is a large, complex, frequently inconsistent system of beliefs. I can briefly characterize them, however, by describing their core components.

Militant Anti-Communism. Proponents of MAC argue that communism is a unitary and implacable enemy of freedom and the West. All of the major ills in the world are attributed to international communism under Soviet leadership. There is no room, in this view, for long-term rapprochement with the enemy. In addition, divisions within communism are unlikely. Thus, there is no reason to court disgruntled communist leaders to weaken the Soviet hold on the Communist movement. There is also a clear image of a world divided strategically into a center and a periphery. Those nations that are strategically central must be defended "at all costs." But because the strongest version of the domino theory is core to the paradigm, even strategically peripheral locations must be defended if threatened. Otherwise there is great danger that a series of "dominoes" will fall and threaten some intrinsically central location. A third set of core beliefs for the MAC paradigm revolve around the "bully metaphor" and derive from a counterfactual reconstruction of the appeasement of Hitler by the Allies at Munich. According to this view, Hitler could have been stopped had the Allies stood up to him. International aggressors, like bullies, are basically cowardly; they will back down from firm opposition. Thus, MAC generally prescribes prompt, decisive, unambiguous, and firm actions.

Pragmatic Anti-Communism. Proponents of PAC believe that totalitarianism is the enemy. Communism is not necessarily monolithic; thus, individual communist leaders like Marshall Tito of Yugoslavia may be split off from Moscow's influence. In addition, PAC argues that the spread of communist/totalitarian movements primarily stems from basic social and economic inequalities rather than from military pressure. The PAC paradigm sees a more subtle form of domino theory rooted in the vulnerability of struggling nations that have been recently decolonized. It follows that the best defense against encroaching totalitarianism is a remedy for these social and economic ills. The PAC paradigm advocates economic and material solutions to most problems, but it is willing to use military power when allies and interests are directly and militarily threatened.

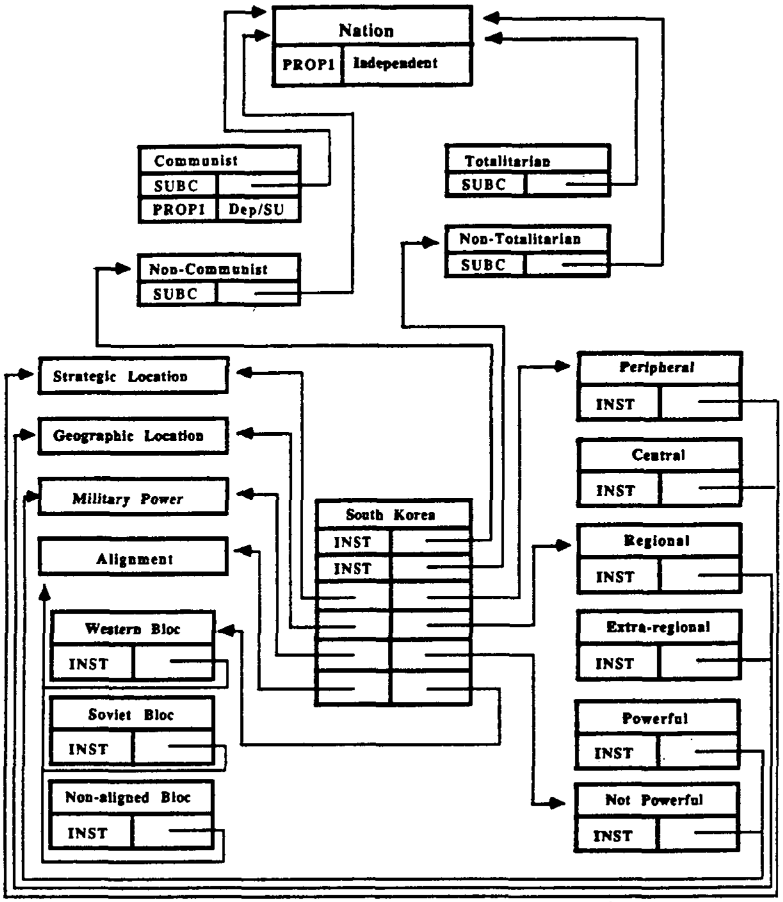
Isolationism. Isolationists argue that the United States should again withdraw from international politics. There are two basic components—one practical, the other moral. In practical terms, the ISO paradigm argues that the United States does not have the resources to maintain world leadership without degrading the quality of life at home. The concern of U.S. leaders should thus be internal. In addition, it would be imprudent to waste resources on a long struggle with communism. In moral terms, the United States can only maintain the ethical high ground by refusing to become involved in international adventures. Any appearance of complicity in colonialism, in particular, would damage U.S. integrity.

Paradigm Structure. Each paradigm in the knowledge base contains two types of knowledge. Procedural knowledge, in the form of IF→THEN production rules, codes how something is done ("IF communists invade South Korea, THEN send military forces"). Declarative knowledge describes important concepts and their relationships ("France is an ally"). These two types of knowledge in a paradigm serve distinct functions in the model: declarative knowledge is used to interpret input events, while procedural knowledge is used to reason from the interpreted events to conclusions.

The interpretive beliefs necessary to a paradigm (i.e., its declarative knowledge) are represented in POLI by a *frame system* with inheritance. One can think of frame systems of knowledge representation as hierarchical relational data bases. Each object in the system (in this case, each regional actor, class of actors, or property of actors) occupies a frame that may have any number of slots representing properties of the object. An object's properties can originate with itself or with other objects ("fillers") to which relational arrows point (i.e., objects "inherit" properties from classes to which they belong). In addition,

FIGURE 1

Militant Anticommunist (MAC) Frame System for South Korea


Nation
PROP1
Independent
Communist
SUBC
PROP1
Dep/SU
Totalitarian
SUBC
Non-Totalitarian
SUBC
Non-Communist
SUBC
Strategic Location
Geographic Location
Military Power
Alignment
South Korea
INST
INST
Peripheral
INST
Central
INST
Regional
INST
Extra-regional
INST
Western Bloc
INST
Soviet Bloc
INST
Non-aligned Bloc
INST
Powerful
INST
Not Powerful
INST

Note: PROP1 is the first in a list of properties of the object; SUBC means the object is a subclass of the category to which an arrow points; and INST means the object is an instance of the category to which an arrow points.

since this information is belief, rather than fact, each object's "degree of membership" in superclasses and the degree of confidence that the paradigm has in its assignment of characteristics must be considered. An empirically derived certainty factor is assigned to each element of belief in the frame.

Figure 1 gives an abbreviated example of interpretive beliefs as a frame system without CFs specified. Of course, this information is actually linked in a large frame system with all other regional actors, though it is difficult to present even two nations on one page clearly, let alone the entire system.

The *production rules* in a paradigm allow reasoning from an interpreted event to some policy output through inference chaining. These rules take the general form of IF→THEN propositions. Some take situations as their conditions ("IF an important enemy attacks an important friend"), while others take goals as their conditions ("IF you wish to oppose the attacker"). In addition, some give goals as their consequences ("THEN oppose the attacker"), while others give executable actions ("THEN attack the attacker"). The bulk of each paradigm consists of a long list of these propositions. Of course, like the declarative information in the frame system, procedural information is uncertain; each rule has an empirically derived CF that indexes the paradigm's confidence in the rule.

POLI'S INFERENCE ENGINE

The inference engine applies beliefs as represented in the knowledge base when POLI is confronted with an event. Processing begins when information about an event (e.g., North Korea attacks South Korea in 1950) is placed in working memory (through the user interface). The inference engine uses declarative beliefs to interpret the event, procedural beliefs to reason forward from its interpretation to a set of possible policies for responding to the event, and then selects a subset of these policies for implementation.

The Interpretation of Events

Suppose I present POLI with an event involving South Korea. The inference engine will search the paradigms in its knowledge base for declarative information about South Korea on which to base its interpretation. It will "understand" South Korea to be the collection of attributes assigned through the declarative information. Of course, like human experts, POLI is most efficient when it has a lot of interpretive information; but POLI does degrade gracefully, producing limited interpretations and inferences from partially understood events.

Consider POLI's interpretation of South Korea based on the abridged MAC frame system in Figure 1. Note first that South Korea is an instance of a noncommunist nation. Since noncommunist nations are a subclass of nations, South Korea will inherit the properties of nations (e.g., independence as an actor). South Korea is also listed as a nontotalitarian nation and has the properties Strategic Location = Peripheral, Geographic Location = Regional, Military Power = Not Powerful, and Alignment = West Bloc. In addition, each piece of declarative information has an associated CF, which the inference engine can use to gauge how much confidence it should place in an interpretive belief. The MAC paradigm might be very confident that South Korea is weak (CF high), but have little confidence in the inference that South Korea is strategically peripheral (CF low). Other information that the paradigm considers important is represented in the complete frame system (e.g., trade relations and the existence of an insurgency movement).

Similarly, each paradigm in POLI interprets the entire event, broken into its components: actor, target, verb, and context. When POLI is unable to interpret part of an event (say, the verb is unrecognized), it alerts the user to the problem and proceeds on the basis of the information it can interpret.

Inference Chaining and Fuzzy Reasoning

A paradigm recommends specific executable behaviors by reasoning through an inference chain from the interpreted event to the output policy recommendation. This inference process begins when the conditions of an IF→THEN rule match "facts" in working memory (initially, just the interpreted event). The rule "fires," and the consequences of the rule are added to the list of known facts. These inferred facts may enable other rules to fire; and, in a cascading effect, more beliefs may be inferred in response to the event, triggering still more rules.

Since I am interested in the diverse arguments that might emerge from these paradigms, there is little restraint in POLI on the growth of inference chains; I want all possible arguments to develop. To avoid infinite looping, no rules can appear twice in the same chain. Otherwise, inference chains grow as long as rules continue to fire. As you might imagine, POLI needs some mechanism for distinguishing among the hundreds of inference chains that can grow from a paradigm. In addition, the multiple paradigms in POLI produce competing outputs and arguments. POLI resolves conflicts of this sort through fuzzy reasoning based on CFs. The inference engine calculates a certainty factor for each of the competing conclusions, allowing POLI to distinguish strong from weak inference chains.

Reasoning in POLI is fuzzy in the technical sense of using fuzzy logic. Based on Zadeh's (1965) original theory of fuzzy sets, fuzzy logic provides a means of formally representing degrees of belief. The membership function of a fuzzy set maps to the [0, 1] segment of the real line, rather than to the two element set (0, 1), which allows a continuum of membership values (Cioffi-Revilla 1981; Negoita and Ralescu 1987; Seitz 1985; and my earlier work, Taber 1991). In fuzzy logic, this property is applied to capture the partial truth value of conditional statements and the natural vagueness of declarative concepts. The conditions in a production rule and the rule itself carry partial truth value. In Boolean logic, based on the theory of crisp sets, such conditions must be either true or false, leaving no room for degrees of truth. This would be a serious inflexibility in a belief system such as POLI. Decision makers clearly have uncertain—sometimes conflicting—beliefs; and they frequently use verbal hedges to qualify their beliefs. A descriptively accurate model of this process must allow reasoning from partial truth or vague meaning.

Surprisingly, given the importance of uncertainty in political beliefs, most of the major political computational models do not address the issue; the notable exceptions are JESSE (Sylvan, Goel, and Chandrasekaran 1990) and the Superpower Rivalry models (Seitz 1987). In the larger context of cognitive science, however, the manipulation of vague and subjective knowledge has been a major focus (Negoita and Ralescu 1987). Several formalisms exist for representing uncertainty, including probability theory and fuzzy set theory.

Where one wishes to express the vagueness of a natural language concept or belief, the approach based on fuzzy set theory enjoys an important advantage over a probabilistic approach (Cioffi-Revilla 1981; Seitz 1985). Consider the concept *powerful.* Its intuitive meaning concerns some vaguely defined quality that actors may have more or less of. But probability theory, based on the mathematics of crisp sets would force us to define some ideal or absolute meaning for the concept. To say, "China is quite powerful" would be to say that there is some fairly high probability that China has this ideal quality *in full.* This is quite different from the natural meaning of the phrase. Fuzzy set theory, on the other hand, focuses our attention on the relative *amounts* of this quality nations have. Now to say, "China is quite powerful" is to say that China, *relative to other actors* has a substantial amount of this quality. Indeed, we can assign a numerical value to the fuzzy variable *powerful* for China that will be understood in relation to the values assigned to other actors. In the POLI project these CFs are empirically derived as part of the content analysis I have described. They are based upon the degree of support for a particular belief within the given paradigm.[9]

The production rules in POLI are fuzzy conditional statements in that certainty factors (CFs) are associated with the elements of the rules. Here I will simply illustrate the algorithm for fuzzy inferencing used in POLI (the derivation is presented in Taber 1991). Consider the rule "IF a nation has large oil reserves, THEN that nation is rich," which has a CF of .3. This rule means that the expert system has .3 confidence in the conclusion "That nation is rich" when it is certainly true that "that nation has large oil reserves" (i.e., when the CF for the premise is 1.0). When the system receives information that "Saudi Arabia has large oil reserves" with CF = .9, we can infer the information that "Saudi Arabia is rich" with a calculable degree of belief. Through an application of the compositional rule of inference, we compare the CF of the new information (.9) with the CF of the known relationship (.3), taking the smaller. The expert system would report the inference that Saudi Arabia is rich with CF = .3. Now if we input the information that "Bangladesh has large oil reserves" with CF = .01, the expert system would only have .01 confidence in the inference that Bangladesh is rich.

Taking a more complex example from POLI, the event "The Vietminh attack France at Dienbienphu in 1954" is interpreted (in part) by MAC as "unimportant enemy (.6) attacks (.9) important friend (.4)." In addition, the MAC paradigm includes the very uncertain production rule, "IF enemy attacks important friend, THEN drop an atomic bomb on the enemy (.08)."

Since in fuzzy logic, conjunctions of fuzzy elements carry the truth value of the minimum fuzzy element and disjunctions carry the truth value of the maximum fuzzy element, the condition of the rule in the example will have a global CF of .4, the minimum of the truth values for the three elements of the premise. But this rule has a very low CF (.08) associated with it, and this low CF must affect the conclusion. Using the compositional rule of fuzzy inference, the smaller of the rule CF and the premise CF is assigned to the inferred conclusion, resulting in a .08 level of confidence in the recommended action, "Drop an atomic bomb on the Vietminh."

These fuzzy algorithms make intuitive sense. *All* the elements must have some truth value for a conjunctive argument to hold; so a conjunctive argument is only as strong as its weakest link. Conversely, a disjunctive argument should be as strong as its strongest element, since *only one* of the elements need be true. Finally, in the context relevant to POLI, where rule CFs define the inferential relationship between premises and conclusions, one may view the compositional rule of fuzzy inference as a special case of conjunction. It is the logical conjunction "Premise is true (CF)" *and* "Rule is true (CF)." Both must have some truth value for the conclusion to have any truth value.

The processor must still select among the recommendations of all of the paradigms. Recall that POLI's inference engine produces three sets of uncertain recommendations—one for each paradigm. To the decision-making system, paradigms are more or less true descriptions of the world at any one time. They are informal theories about regional processes. One paradigm may dominate discussion among policymakers for a period because the decision-making system believes it to be a good representation of the world. Other paradigms still create arguments and inferences, but their individual arguments cannot be more persuasive than the paradigm as a whole. POLI limits the confidence it places in particular inferences of a paradigm to the confidence it places in the paradigm. Again, following the rule of fuzzy conjunction, POLI assigns to each recommendation of a paradigm the minimum of the paradigm's confidence in the recommendation and POLI's confidence in the paradigm itself.

Recall the Dienbienphu example that resulted in the inference "Drop an atomic bomb on the enemy" with CF = .08. Even if the paradigm that makes this recommendation is quite dominant (as MAC was in 1954), POLI would only accept this inference with .08 confidence—the CF of the inference *within* the MAC paradigm. Now consider the weaker isolationist paradigm (CF = .025). Even if it made an inference of which it was quite sure—"Remain neutral (.9)"—the inference would only be accepted by POLI (*outside* of the ISO paradigm) with a .025 CF.

An input event, then, produces several recommended policies from the paradigms, each with a CF attached, and each with supporting arguments. POLI selects a subset of these recommendations upon which to act, using the CFs as a guide. One selection mechanism in POLI generates a probability distribution over the CFs of all policies in the recommendation set and chooses policies stochastically. In the Dienbienphu example, though neither ISO's recommendation to remain neutral nor MAC's atomic policy would be very probable, there would still be a small chance of each. A second approach deterministically selects the recommendations with the highest CFs.

VALIDITY AND POLI AS AN ANALYTIC TOOL

Computational modeling techniques allow foreign policy analysts to increase the descriptive detail of their models without losing analytic focus. I shall demonstrate the descriptive complexity possible using the Policy Arguer as an analytic tool while also addressing the issue of validity.

In discussing an information-processing model like POLI, we must distinguish outcome from process validity, where outcome validity is the familiar notion of correspondence between the model's predictions and behavior from the real world system. Do the outputs of the information-processing model—treated as a blackbox—match, to an acceptable level of statistical significance, the outputs of the empirical system? Process validity, on the other hand, concerns the correspondence between the mechanisms in the model and those in the real world system. If an important virtue of a computational model is that it is *not* a blackbox, we must demonstrate its reasonableness as a process model.

Outcome Validity

The outcome validity of POLI was checked using a chronology of Asian events in the 1950s, compiled by the Royal Institute of International Affairs, as a test data set ($N = 161$). For each event in the test data, the actor, target, and verb were identified, along with the U.S. responses. An average of slightly more than three responses per event were given in the test data. It was then straightforward to run each event through POLI and compare the recommended policies with the policies actually chosen by the United States. If the historically selected policy or policies were highly recommended by POLI, where a "high recommendation" was operationally defined as "having one of the top five CFs in the recommendation set," it was counted a success for the model.[10] If *all* of the actual U.S. responses identified in the test data set were not highly recommended by POLI, it was counted a failure for the model.

POLI did remarkably well in the decade of the 1950s, succeeding on 138 out of the 161 events. For just under 86% of the events in the test data set, POLI's highest recommendations included all of the policies chosen by the U.S. foreign-policy-making system. In addition, an analysis of the 23 failures shows that POLI came close for most of them. Either a chosen policy was just outside the highly recommended set and would still be a likely choice if the selection were stochastic or else the real world policy was too imperfect a match with POLI's recommendation. For example, POLI (based on MAC reasoning) highly recommends that the United States accuse China of imperialism in 1953 in response to China's attempt to increase trade with Great Britain. The actual policy chosen by the United States, as identified in the test data set, was "attempt to block increased trade." This is not a match, but one could argue that the intention is the same. In addition, an exact match in this example fell just outside the set of highly recommended policies. MAC pushes the policy "oppose increased trade between China and Great Britain," but only with .17 confidence; the cutoff for high recommendation was .282 in this case.

A potential problem exists with this analysis, however. The knowledge and beliefs that POLI uses to interpret events and reason to conclusions are extracted from speeches in Congress in the same time period as the events. Although the test data set is operationally independent of the *Congressional Record,* some of the speeches and articles respond directly to particular events that appear in the test data set. In other words, the logic and arguments on which POLI is based are, in part, the congressional responses to events in Asia. Because these events do appear in the test data set, there is a possibility of circularity.

To find out if this problem was serious, I used the knowledge base from the prior period to operate on events. Thus, I used the 1949–52 knowledge base for 1953–56 events, the 1953–56 knowledge base for 1957–60 events, and the 1957–60 knowledge base for 1961–63 events.[11] POLI remained very successful, even for this more stringent test. Fully 75% (84/112) of the actual U.S. responses to the events matched POLI's highest recommendations.

But how should we assess the success of POLI? Is 86% correspondence "good"? To gauge POLI's success, I will compare it with the performance of two null, or baseline, models: the *inertia model,* where existing policies are repeated in response to new events, and the *modal behavior model,* where frequently used policies are selected in response to new events. These null models are variants of the well-known organizational process model (Allison 1971), and we might expect them to do reasonably well. I will instantiate the two null models using the chronology of Asian events as described and compare POLI's performance with each.

But if I count successes and failures for the models at the descriptive level I have used for POLI, neither of the null models can approach POLI's performance. POLI matches actual U.S. responses to Asian events in the test data in much greater detail. In addition, neither of the null models can interpret events. The events must be categorized for them. POLI's task, which includes the interpretation of "raw" events and the detailed recommendation of policy responses, is considerably more comprehensive than either of the null models can attempt. Indeed, the fact that POLI does more theoretical work is already a powerful recommendation.

Table 1 presents the coding scheme used to reduce the level of description of the test data set. Quantifying event types and U.S. responses gives us a more typical events data set,[12] allowing a fairer test of the null models and a clearer comparison with POLI. Each of the 161 events in the test data set was placed in one of the eight categories, where plus signs denote a cooperative verb, minus signs, a conflictual verb. Thus, when Soviet leaders visited India in 1955, promising development assistance, it was a type 4 event: an enemy of the United States did something cooperative to a friend of the United States. Next, the U.S. response to each event was coded according to the ordinal scale in Table 1. Since this response is directed, we also code whether it is aimed at the actor in the event (1), the target in the event (2), or a third party (3). For example, the United States increased its development assistance to India to counter the 1955 Soviet offer. This response is coded a 2–2, the first digit indicating the position of the response on the scale, and the second, that it was directed at the target in the original event (India). In addition, the United States denounced the Soviet Union as impe-

TABLE 1

The Categories of Behavior

Event Types

1. Enemy + Enemy
2. Enemy − Enemy
3. Enemy + Friend
4. Enemy − Friend
5. Friend + Friend
6. Friend − Friend
7. Friend + Enemy
8. Friend − Enemy

U.S. Response Categories

1. Military Aid
2. Economic/Development Aid
3. Verbal Support
4. Positive Propaganda (e.g., symbolic support for nationalism)
5. Persuade, Negotiate
6. Negative Propaganda (e.g., name calling)
7. Verbal Condemnation (e.g., diplomatic protests)
8. Verbal Threat
9. Nonverbal Threat (e.g., movement of troops)
10. Direct Military Force

TABLE 2

POLI's Predictive Accuracy Compared with Several Null Models

MODELS	CORRECT	INCORRECT	% CORRECT
POLI	143	18	88.8
Sophisticated modal	105	56	65.2
Naive modal	75	86	46.6
Modal inertia	66	87	43.1
Inertia	65	88	42.5

Note: Each of the five models makes predictions about the U.S. response to events in Asia in the 1950s, which are tested against the actual behavior of the United States as compiled by the Royal Institute of International Affairs (N = 161). The inertia models do not generate a prediction the first time an event type is encountered (N = 153).

rialistic, which would be coded a 6–1: a 6 on the response scale and a 1 indicating that the response was directed at the actor in the event (the Soviet Union).

Since it gives POLI a major advantage in the test, I do not consider more than one U.S. response to each event in the test data set. Rather, I will only include the most extreme U.S. response. For each event, the U.S. response closest to an endpoint on the scale was coded. Note that this gives the null models their best chance of producing successful predictions in the data.

According to the inertia model, the United States will continue its existing policy given a particular type of event. For example, when the United States is confronted with a type 4 event, it searches history for the most recent occurence of a type 4 event and repeats the policy it used then.

The modal model says that the United States will apply its most frequently used policies in response to an event. A naive modal model might only consider the single most frequent response for each type of event. For example, the modal response for a type 4 event is a verbal condemnation of the actor (7–1). Every time a type 4 event occurs, the naive modal model selects 7–1. A more sophisticated modal model, on the other hand, would consider the two or three most common responses to each event. In this case, the model would propose, in response to a type 4 event, *either* a 7–1 or a 1–2 (military aid to the target). If either of these was the actual U.S. response, it would be a success for the sophisticated modal model.

We should expect the modal models to do very well in that they apply future information in selecting a response—they provide more hindsight than prediction. Of course, as I have noted, POLI also applies some beliefs developed on the basis of current events, though the circularity is not nearly as direct as in the modal model. POLI, of course, remained very successful even when this potential circularity was eliminated (75% success). As a final null model, we will use a version of the modal model that does not consider future information. The modal intertia model applies the most frequent responses to events of a given type from history at the time of the event. Instead of doing a frequency count across the decade for responses to each event, we now consider only the frequencies of responses that have occurred before the event. Thus, response 1–2 was the modal inertia response to a type 4 event in January of 1950; but by 1955, the modal inertia response was either 7–1 or 1–2.

Finally, POLI's responses to the events were coded using the same scale. Since POLI recommends multiple responses, only the one with the highest CF was coded. If several policies tied for the highest CF, the most extreme response was used.

Table 2 directly compares the four null models and POLI. Note first that POLI improves a little on its earlier performance, because the test is less stringent. Some of the 23 misses in the earlier test are matches at this level of description. The sophisticated modal model also does fairly well (65%), though POLI's proportion of successful matches is significantly better. Using the best null model (the sophisticated modal model) as a baseline for success in predicting the historical trace, the probability of achieving POLI's success if it were not in fact a better correspondence model is very small ($p < .00001$ using the binomial test). POLI clearly outperforms the null models.

Process Validity. It should be clear, of course, that POLI makes many recommendations for each event that are not close to the actual responses of the U.S. government. This is not a defect. The Policy Arguer is a process model of the decision-making system and not just of policy outcomes. It models the diverse arguments in the system, even the ones that are not acted upon. The previous analysis demonstrated that most of the time, POLI's strongest recommendations are the things that the United States actually did. But a more compelling concern is the process by which those outcomes are reached.

In many respects, the most interesting feature of POLI is the detailed argumentation that supports its recommendations. This section recreates a case study of the U.S. response to the outbreak of the Korean War. Though we focus on this one case here, a similar case study emerges from every POLI run.

Unfortunately, detailed case studies can do no more than establish the process *plausibility* of POLI. As others have convincingly argued, correspondence tests of process generally are a species of face validity, requiring a different mode of analysis than the familiar statistical approach to outcome validity (Job and Johnson 1991; Schrodt 1991; Sylvan, Goel, and Chandrasekaran 1990; Whicker and Sigelman 1991). I will support the face validity of POLI as a process model by detailing POLI's response to the Korean case and comparing it to the reasoning and arguments attributed to the United States in historical case studies.

On June 25, 1950 troops of the North Korean army crossed the border between North and South Korea in a massive surprise attack. Over the next few days, U.S. leaders made three key decisions concerning this invasion: (1) that the United States had to intervene militarily, despite earlier assurances that South Korea (and even Taiwan) was outside of the U.S. defense perimeter in Asia; (2) that they would use the collective security mechanisms set up in the United Nations Charter; and (3) that the intervention must be limited in nature.

When POLI is confronted with the event "North Korea attacks South Korea in 1950," each of the three paradigms interprets the event and then reasons forward from the interpreted event to a set of policy options (Table 3). The trace of this process provides POLI's explanation of each policy.

The militant anticommunist paradigm (MAC) characterizes North and South Korea according to the attributes that the paradigm considers important. For example, MAC believes that both Koreas are strategically peripheral, that North Korea is East-aligned and South Korea West-aligned, and that both Koreas are militarily weak (though MAC is far more confident that South Korea is weak). The values of other attributes are also assigned from the MAC knowledge base. Several basic interpretive rules then operate on this declarative information to classify North and South Korea as unimportant expansionist enemy and unimportant friend, respectively. Finally, MAC interprets the verb *attack* to mean "physically threaten." Of course, MAC's real understanding of *attack* is found in the way its rules actually use the verb *physically threaten*.

Given MAC's interpretation of the actor, target, and verb, POLI can now apply its procedural knowledge to infer courses of action in response to the event. The following display provides the MAC reasoning behind the decision to intervene militarily:

TABLE 3

POLI's Policy Options for the 1950 North Korean Attack

PARADIGM[a]	POLICY OPTIONS	CERTAINTY FACTOR[b]
MAC	Communicate commitment to protect target	.560
	Increase military aid to target	.560
	Increase military support to regional non-communists	.500
	Military blockade on actor	.500
	Send military advisors to target	.500
	Act through the U.N.	.440
	Send U.S. troops to target	.440
	Investigate the event/problem	.440
	Trade embargo on actor	.440
	Reduce aid to actor	.400
	Communicate opposition to regional hegemony	.400
	Aid friends in region who oppose actor	.350
	Diplomatic sanctions against actor	.350
	Increase material support to regional non-communists	.330
	Increase trade with target	.280
	Increase economic aid to target	.280
	Increase industrial aid to target	.270
	Increase global military readiness	.220
	Communicate the containment line in Asia	.220
	Threaten use of atomic bomb	.210
	Directly attack communist actors in Asia	.170
	Subvert communist actors in Asia	.170
	Do nothing	.110
	Withdraw diplomatic recognition of actor	.110
	Withdraw envoys	.110
	Pressure allies to sanction actor	.110
	Consult allies about intended policy	.110
	Lodge protest in U.N.	.056
	Link economic aid to target compliance on other issues	.056
	Attack actor	.000
PAC	Avoid direct U.S. military involvement in Asia	.250
	Consult with Asian leaders before taking action	.250
	Sponsor conferences to gather input from Asian leaders	.250
	Restrict direct use of U.S. ground troops	.250
	Apprise Soviets of limited U.S. objectives	.250
	Do not directly attack Soviet territory	.250
	Raise problem in U.N. Security Council	.250
	Support only popular governments in Asia	.250
	Promote individual freedoms and human rights in Asia	.250
	Help Asian leaders address immediate welfare concerns	.250
	Help aspiring Asian democrats to write constitutions	.250
	Accuse Soviet communism of imperialism in Asia	.250
	If aid is given, do not attach strings to the aid	.250
	Encourage civil liberties in target	.250
	Monitor use of U.S. aid by target	.250
	If target is to be aided, give slowly so target can absorb it	.250
	Increase educational aid to target	.250
	Increase economic aid to target	.250
	Increase technical aid to target	.250
	Promote principles of majority rule, freedom, and equality	.250
	Publicly repudiate colonialism	.250
	Do not approach Soviet territory in military action	.240
	Apprise China of limited U.S. objectives	.230
	Do not directly attack Chinese territory	.230
	Do not approach Chinese territory in military action	.230
	Consider minimal use of air power first	.210
	Publicly support target	.140
	Encourage Asian nationalists to redistribute wealth	.140
	Tolerate independent foreign policies of Asian allies	.140
	Increase humanitarian aid to target	.140
	Freeze military aid to target	.110
ISO	Remain neutral about event	.125
	Offer to mediate dispute between actor and target	.125
	Reduce defense commitments in Asia	.125
	Rely on air and nuclear forces for defense of U.S.	.125
	Withdraw U.S. forces from Asia	.125
	Do not get involved in the internal affairs of Asian nations	.125
	Withdraw from U.N. Security Council	.125
	Reduce U.S. financial commitment to U.N.	.125
	Ask U.N. to leave New York	.125
	Do not join major U.N. peacekeeping efforts	.125

[a] The paradigms are militant anti-communism (MAC), pragmatic anticommunism (PAC), and isolationism (ISO).

[b] These certainty factors (CFs) represent the confidence POLI has that the given policy option would be a good choice. They are based on the empirically-derived CFs for the expert knowledge and the fuzzy inferencing techniques described above.

IF Actor physically threatens target
THEN Actor is important (.6)
AND Target is important (.6)

IF Political system of actor is communist
AND Political system of target is non-communist
AND Actor and target are contiguous
AND Actor physically threatens target
THEN Target is in danger of falling communist (.9)

IF Target is friend
AND Target is important
AND Target is in danger of falling communist
THEN Goal: Prevent target from falling communist (.6)

IF Goal: Prevent target from falling communist
THEN Goal: Reinforce military preparedness of target (.56)

IF Goal: Reinforce military preparedness of target
THEN Policy: Increase military aid to target (.56)
AND Policy: Send military advisors to target (.5)
AND Policy: Send U.S. troops to target (.44)

One of the features of the MAC paradigm is that targets and actors take on new importance when they are active. This comes from the belief that all parts of the world are linked together, so that peripheral nations become the front line when they are threatened (or when they threaten). This was one reason that U.S. leaders explicitly led North Korea and the Soviet Union to believe that South Korea was outside the defensive perimeter of the United States yet responded vigorously to the attack when it happened (Paige 1968, 66–7). Until the attack occurred, Korea *was* outside the U.S. defense perimeter. The belief that peripheral nations take on new importance when militarily active—an essential component of the domino theory—is represented in MAC by the first rule in the display.

The second rule represents another key part of the domino theory: noncommunist nations that border communist nations are in great danger of falling whenever they are the target of communist aggression. Thus, a cascade of collapses can be expected if any nation is allowed to fall. United States leaders used this line of reasoning as their major argument for intervention (George and Smoke 1974, 162–72; Glad and Taber 1991; Nathan and Oliver 1985, 120; Paige 1968). In his *Memoirs*, for example, Harry Truman wrote: "I felt certain that if South Korea was allowed to fall Communist, leaders would be emboldened to override nations closer to our own shores. If the communists were permitted to force their way into the Republic of Korea without opposition from the free world, *no small nation would have the courage to resist threats and aggression by stronger Communist neighbors*" (1956, 333, emphasis mine).

The third rule in the display sets up the goal "Prevent target from falling communist" in response to the facts in working memory: "Target is friend," "Target is important," and "Target is in danger of falling communist." In order to achieve this goal, several subgoals are set up, including the one given in rule 4. Finally, rule 5 operates on this subgoal by proposing three policies: "Increase military aid to target," "Send military advisors to target," and "Send U.S. troops to target." All three of these specific forms of military intervention, recommended by MAC, were actually taken by the United States in response to the North Korean invasion of South Korea in 1950. More importantly, the reasoning found in the display corresponds to the reasoning in the system—or at least to public justifications (Paige 1968; M. Truman 1973, 504–10).

MAC follows several other lines of reasoning to arrive at the policy recommendation "Act through the UN." One is related to the costliness of sending U.S. troops into action. By acting under UN auspices, the United States could keep economic and psychological costs down. This, of course, was part of the concern for limiting the war. Public opinion was strongly opposed to any long-term and bloody intervention (Nathan and Oliver 1985, 121–27; M. Truman 1973, 508–16). Thus, part of the reason for acting under UN auspices was reducing the potential cost to the United States.

A second reason offered by POLI (MAC) for acting through the UN is the principle of collective security. Policymakers of this period, both from the MAC and the PAC paradigms, believed that the proper response to any international aggression which violates international law, is collective. This was one of the primary reasons for the existence of the UN and its Security Council. MAC develops this argument in full—though in MAC it applies only to friends. It would not be triggered if the target of aggression was communist.

The collective security argument was prevalent among the U.S. decision makers reacting to the North Korean invasion (Paige 1968; M. Truman 1973, 498–510). One of the earliest communications from General MacArthur in Japan (Commander of U.S. Pacific Forces) on the morning of 25 June set the tone by calling the North Korean attack an "undisguised act of war *subject to United Nations censure*" (M. Truman 1973, 496, my emphasis). Indeed, Secretary of State Dean Acheson moved to bring the case before the UN Security Council even before notifying President Truman, who was in Missouri on family business (Nathan and Oliver 1985, 120).

Similar sets of arguments can be made about the other MAC recommendations found in Table 3. For example, the strong inference "Communicate commitment to protect target" follows from the belief "Target is friend" and from the domino theory conclusion "Target is in danger of falling communist." Whenever friends are in danger of toppling, according to strong elements of MAC, the commitment to protect them should be expressed publicly.

Although the confidence POLI has in the pragmatic anticommunist paradigm (PAC) is only slightly more than half as great as the confidence it has in MAC, PAC does make important contributions to the policy debate. The PAC interpretation of the event is very similar to the MAC interpretation. North and South Korea have the same basic characteristics, but PAC attaches different CFs. For example, PAC is less confident that North and South Korea are strategically peripheral. Given this interpretation of the event, POLI now uses the PAC rule base to produce another, and competing, set of policy recommendations (see Table 3).

One line of argument supports the MAC view that action should be taken in the UN. This reasoning parallels the collective security argument just discussed. It is found in the rule,

IF Actor physically threatens target
AND Actor is not US
THEN Policy: raise problem in UN Security Council (.44).[13]

According to this rule, whenever there is international aggression that the United States did not initiate, the issue should be taken to the UN Security Council and dealt with as a collective security problem.

The third major decision made by U.S. leaders in response to the North Korean attack was to limit the U.S. intervention. The MAC paradigm provides a good "replication" of the first two real world decisions—to intervene and to act through the UN. But the only line of reasoning MAC offers relevant to this third decision is more directly applicable to the decision to act through the UN. The following display presents two inference chains from PAC for this third broad decision:

Chain 1

IF Goal: Contain Soviet communism in region
THEN Goal: Promote Asian nationalism (.44)

IF Goal: Promote Asian nationalism
THEN Policy: Avoid direct U.S. military involvement in Asia (.44)
AND Policy: Consult with Asian leaders before taking action (.44)
AND Policy: Sponsor conferences to gather input from Asian leaders (.44)

Chain 2

IF Actor physically threatens target
AND Borders of actor or target is USSR
AND Goal: Avoid World War III
THEN Goal: Limit U.S. military involvement (.49)
AND Goal: Avoid taking action that threatens Soviet security (.4)

IF Goal: Limit U.S. military involvement
THEN Policy: Restrict direct use of U.S. ground troops (.34)
AND Policy: Consider minimal use of air power first (.21)

IF Goal: Avoid taking action that threatens Soviet security
THEN Policy: Apprise Soviets of limited U.S. objectives (.3)
AND Policy: Do not directly attack Soviet territory (.4)
AND Policy: Do not approach Soviet territory in military action (.24)

PAC argues in Chain 1 that one of the best ways to contain Soviet communism in Asia is to strengthen the indigenous support for Asian leaders. This general goal of containment is one of the paradigm goals for PAC that applies in any situation. PAC adopts several subgoals designed to achieve containment, including the goal "Promote Asian nationalism," which in turn causes the last rule to fire, producing three policy recommendations to limit direct U.S. involvement. These recommendations exemplify the belief that Asian nationalism as an anticommunist force will strengthen if allowed independence. This line of reasoning also recognizes that Asian leaders are an important source of information and understanding about events in Asia.

Several congressional leaders used these arguments as part of their attack on the Truman administration's quick decision to intervene. In the early days of the war, the United States did not consult Asian allies about the proper response to the invasion. This failure to consider Asian sensibilities and expertise was severely criticized in the United States (and abroad) along the lines argued in PAC (LaFeber 1972, 100–121).

Chain 2 represents a different line of argument for limited intervention. Whenever international aggression occurs in the geographic vicinity of the USSR (a similar chain fires for China), PAC becomes very concerned about avoiding a direct confrontation. Special care must be taken not to provoke security concerns for either China or the Soviet Union in these cases.

The specific policy recommendations from Chain 2 were clearly followed by the United States in the first few days after the North Korean attack. A chronology of the decision process presented by Snyder and Paige (1962) demonstrates the accuracy of POLI's reasoning. It includes the choices on June 26 "to commit American air and naval units in direct combat support" of the South Koreans and "not to use ground forces in direct combat at this time"; on June 28 "to extend American air and naval action north of the 38th parallel"; and on June 29 "to restrict use of ground troops to protection of supply lines and evacuation of citizens," "to restrict American air and naval action to purely military targets north of the 38th parallel," and "to order air and naval units to stay clear of the Soviet and Chinese borders" (pp. 237–38). United States leaders also explicitly informed Moscow on June 27 that American objectives in Korea were limited (LaFeber 1972, 99). In addition, the arguments used in the actual decision making system to support these policies were precisely the ones used by PAC (outlined in the text and in the display) to infer them (Paige 1968).

Because POLI has little confidence in the isolationist paradigm, its policy recommendations have significantly lower CFs. The ISO dominance of .125 is assigned to all of its inferences to reflect this skepticism. I will trace two lines of reasoning from ISO, one resulting in the recommendation to "remain neutral" and the other in the recommendation to avoid involvement in "internal affairs of Asian nations."

Unlike MAC, the isolationist paradigm does not invest added importance in militarily active nations. The Koreas were strategically peripheral before the attack, and they remain so after the attack. So the first chain consists of the single rule,

IF Strategic location of actor/target is peripheral.
THEN Policy: remain neutral about event (.7).

The second inference chain responds to the general ISO goal, "Never violate sovereignty." To achieve this goal, ISO recommends the policy, "Do not get involved in the internal affairs of Asian nations."

In fact, only a fraction of American elites objected to the U.S. intervention, and only part of this dissent was motivated by isolationist arguments like these. Senator Taft, one outspoken isolationist critic in 1950, accused the Truman Administration of overstepping its authority by intervening without consulting Congress. In some of his criticisms, Taft used both of the arguments just sketched for ISO (Foster 1983, 103–4). At other times, however, Taft seemed to support the intervention, merely disagreeing with Truman's failure to consult Congress (LaFeber 1972, 99).

Though I certainly cannot claim to have proven POLI's process validity, I do believe the reasonableness, or face validity, of the model to be well supported by this detailed comparison of POLI's reasoning with that of the U.S. policy-making system. In addition, two other case studies generated by POLI (attempts to settle Japan's disputes with South Korea in 1953 and the 1958 Taiwan Straits crisis) have been successfully compared to historical descriptions (Taber 1991).

Sensitivity Analysis. Like many computational models, POLI is very complex, containing three sets of production rules and three large frame systems for declarative information in each time period. An important question in models of this sort is, How sensitive performance is to changes in the knowledge base? (Benfer, Brent, and Furbee 1991). Do the model's reasoning and outcomes change with minor modifications of CFs or when we eliminate some of the rules? A reasonable model should eventually degenerate with the elimination of model components (Whicker and Sigelman 1991); but it should also be robust in the face of minor changes of features of the model about which we are relatively unsure. In other words, we want a model that is sensitive to modifications of central components but less sensitive to changes of peripheral components.

I will test sensitivity in POLI on three levels. First, the model should clearly be sensitive to substantial changes in paradigm dominance. If the relative dominance of the MAC and ISO paradigms flipped in 1950, I would expect a somewhat different response to the Korean invasion than the one we saw. In fact, I do find that systematic variations of the relative dominance of the three paradigms affect model performance. (It would be rather disingenuous of me to applaud this result, however, since it clearly follows from the model structure as described.)

Second, I identified the elements of belief that appear *never* to be used in a chain of reasoning. I ran the 161 events in the test data set through POLI, keeping a counter for the number of times each element of belief was used. Typically, large rule-based systems like POLI contain a significant amount of redundant information. So I was surprised to find that POLI was really quite lean. Only 108 of the 1,467 rules were never active in this test (though a somewhat larger number were rarely active). Note that in addition to estimating the amount of chaff in POLI, I can also use this sensitivity test to establish rigorously the core and periphery of the paradigm. Not surprisingly, there was a strong correspondence between a rule's activity counter and its CF. All 108 inactive rules had CFs too small ever to lead to a highly recommended policy.

Third, the model should be sensitive to major changes in the relative CFs of core and peripheral beliefs in a paradigm; but it should not be sensitive to minor changes in these CFs, especially when the relatively crude empirical basis of these CFs is considered. I checked this sensitivity by systematically varying the CFs in the MAC paradigm while monitoring the policy recommendation set. Because this procedure is computationally very intensive, I have not repeated it for the other two paradigms. The broad results for MAC should generalize, since the three paradigms were produced in the same way.

Each rule in MAC was tested by varying its CF while all other elements were held constant at their empirically derived confidence levels. Eight events, one of each type from Table 1, were run through the paradigm for each set of CF values. The purpose of this sensitivity test is to estimate the amount of change in CFs needed to alter the set of recommended policies. Three broad results emerged: (1) averaging across all rules, a fairly substantial change is needed in a single rule's CF to alter the policies in the recommendation set (±.26);[14] (2) the recommendation set is much more sensitive to core beliefs than to peripheral beliefs; and (3) the behavior of the model changes *completely* when the rule CFs are inverted (i.e., subtracted from 1.0). These results are a very nice fit with the general desiderata for sensitivity in a model like POLI. As we would wish, POLI is sensitive to major changes in the CFs without being highly sensitive to minor changes. In addition, the distinction between core and peripheral beliefs has a meaningful impact on output.

POLI on the New Frontier of Computational Modeling

The detail with which POLI is able to replicate traditional historical case studies of U.S. foreign policy decision making is a powerful argument for its validity as a descriptive model of the decision-making system. While maintaining analytic focus in the sense that all behavior within the model is fully understood, POLI achieves a very high level of descriptive realism. In particular, POLI represents foreign policy making as a complete process rooted in prior beliefs and controversy.

Perhaps the most important contribution of the Policy Arguer project is that it demonstrates the utility of the computational approach for foreign policy analysis. The computational modeling techniques described in this work (along with many others that were not appropriate for POLI) offer tremendous opportunities for political science analysts, who must frequently deal with large complex systems based on either natural language communication or symbolic processes generally. Computational modeling techniques allow the information-processing black box to be opened and modeled in great detail and in an intuitive way. Natural language systems resonate with our common notions of how the black box really works. In this way, formal analyses can approach the descriptive complexity of traditional analyses.

In addition, computational modeling provides a platform for controlled experimentation. To the extent that a model like POLI is a reasonable descriptive representation, it can be used to simulate the behavior of the real system. This allows a degree of manipulation and control impossible with real political systems. In particular, counterfactual simulation using computational models promises to become a useful mode of inquiry in foreign policy analysis (Taber 1991; Thorsen and Sylvan 1982). Sometimes the things that did not happen or the decisions that were not made become very important in understanding a historical case.

Finally, computational modeling techniques are now readily available to a large number of users at fairly low cost. One does not have to be an experienced programmer or have a mountain of equipment to use these techniques. (POLI I, for example, was developed on an IBM-compatible personal computer and runs easily within 640k, using less than 1.5 megabytes of disk storage space.)

The generality of the techniques used in POLI allows them to be widely used by nonprogrammers.[15] Perhaps the success of POLI and other recent computational models will inspire other political scientists to avail themselves of this new method.

Notes

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1. The operational code approach has commonly been used to study particular leaders and how their belief systems influence their decision making. Ramsey MacDonald, Getulio Vargas, MacKenzie King, Dean Acheson, John Foster Dulles, Dean Rusk, Arthur Vandenburg, Lyndon B. Johnson, Mao Tse-tung, Liu Chao-chi, Willy Brandt, Kurt Schumacher, Lester Pearson, Pierre Trudeau, J. William Fulbright, and Henry Kissinger have all been subjects of such analyses (Holsti 1977).

2. Francis Crick recently wrote that "elegance and a deep simplicity . . . are useful guides in physics, but in biology such intellectual tools can be very misleading (1988, 6). The same might be said for many problems in the social sciences.

3. To avoid "the AI hype" and controversies over the meaning of *intelligence*, most practitioners prefer the label *computational modeling* to the better known *artificial intelligence*. For a general overview of computational modeling in international relations, including a discussion of this debate see Schrodt 1991. The work of Anderson, Minsky, Newell, Simon, and others in cognitive science will not be reviewed here (though their influence will be readily apparent to all familiar with their work). Several excellent reviews of the cognitive science literature exist, including Collins and Smith 1988, Sharples et al. 1989, and Shapiro 1990.

4. Successful expert systems exist for many domains, including geological exploration and medical diagnosis. See Benfer, Brent, and Furbee 1991 for a discussion of expert systems in the social sciences.

5. POLI was programmed in C on a Dell System 310 personal computer and compiled using Microsoft C 5.1.

6. In other words, POLI takes a situation as input and reasons forward to conclusions. A backward-chaining engine takes conclusions as input and reasons back to the conditions that may have caused the conclusions. I discuss fuzzy inferencing below.

7. Nathan Leites (1951) took a similar approach when he outlined the "operational code" of Bolshevik leaders. This shared belief system was a crucial element in explaining Soviet behavior. See also Majeski 1987, Boynton 1987 and 1991, and Hayward Alker's (1984, 1988) work on historical argumentation in which he formally analyzes the logics used in classical and contemporary political debates.

8. These paradigms are essentially equivalent to a three-part typology of "public attitudes" toward U.S. foreign policy identified by Bardes and Oldendick (1978), Holsti (1979), and Modigliani (1972): hawkish internationalism, dovish internationalism, and isolationism.

9. Actually, despite my strong endorsement of fuzzy inferencing (based on *process validity* concerns) it makes little *practical* difference which approach is taken. To satisfy my curiousity, I programmed POLI to use probabilistic and fuzzy inferencing in parallel and I compared the results (Taber 1991). The differences were minor. In general, fuzzy inferencing produced *slightly* more complex choices for POLI because the policy recommendations were less distinguishable—there were more ties. I report the fuzzy results here because I am convinced that it is a more appropriate representation of the real inferencing process.

10. To identify POLI's highly recommended policies (those with the top five CFs) I used the deterministic selection mechanism described above. POLI identified the five policies (more if there were ties for fifth place) with the highest CFs in the recommendation set.

11. On the other hand, since belief systems are the *cause* of policy output in this analysis, the use of belief systems from an earlier period may not be appropriate in predicting output. In essence, I am now testing the degree of change in belief systems (i.e., how well belief systems from an earlier period predict policies in a later period). It's not clear that we should expect much agreement.

12. These categories are similar to those developed in the

WEIS (Fitzsimmons et al. 1969) and COPDAB (Azar 1972) events data sets.

13. Note that this CF is PAC's confidence, not POLI's.

14. This figure represents the average amount of change in a single rule's CF that was needed to change the set of highly recommended policies *for at least one* of the eight event types.

15. Many of the inferencing techniques are general across problems. Inference engines and user interfaces (called shells) are readily available for the user who does not need special inferencing features. These shells are empty of content. They guide the user through the development of knowledge bases for the problem of interest and through the use of the knowledge bases once developed (Carrico, Girard, and Jones 1989; Martin and Oxman 1988). POLI's inference engine, for example, is a completely general forward-chaining engine. It could be used with any set of IF→THEN rules for many different research problems. The new user interface and inference engine for POLI will eventually be available as a stand-alone shell, which will be superior to commercially available shells for many analytic purposes. In particular, its inference engine employs fuzzy reasoning in a more sophisticated way than commercially available shells, which were designed for decision support, rather than research.

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PUBLIC OPINION AND THE COMMON DEFENSE: WHO GOVERNS MILITARY SPENDING IN THE UNITED STATES?

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We measure the extent to which military spending policy reflects public opinion, while controlling for other reasonable influences on policy. We use survey data as an indicator of aggregate public opinion on military spending and find evidence that changes in public opinion consistently exert an effect on changes in military spending. The influence of public opinion is less important than either Soviet military spending or the gap between U.S. and Soviet military spending and more important than the deficit and the balance of Soviet conflict/cooperation with the United States. We also examine the hypothesis that public opinion does not influence the government but that the government systematically manipulates public opinion. We find no evidence to support this hypothesis.

"Liberal" democracy, or polyarchy, is typically defined procedurally, in terms of the institutions that guarantee freedom of organization and expression, a wide franchise, and competitive and fair elections. These conditions are thought to make government policies depend on the vote, as well as on other expressions of the preferences of the public (Dahl 1971, 1989).

Some scholars have found evidence that democratic institutions in the United States may be decreasingly compatible with democratic norms. Mayhew (1974), for example, points out that congressional races are increasingly dominated by incumbents. Burnham (1982), among others, maps the decline in voter turnout; and Ginsberg (1990) argues that the influence of electoral mechanisms has declined.

Other scholars, however, have found that policy does reflect public opinion, evidence of the representativeness of American government. Ostrom and Marra (1986) find that policymakers seem to consider public support or opposition to increases in military spending (where *public opinion* is defined as a plurality favoring either an increase or decrease in military spending). Page and Shapiro (1983, 1992) find that change in public opinion tends to precede change in policy. They conclude that in a wide variety of issue areas "opinion changes are important causes of policy change" (Page and Shapiro 1983, 189). Other scholars including one of the present authors, Russett, have found similar results (e.g., Jacobson 1985; Risse-Kappen 1991; Russett 1990, chap. 4; Russett and Graham 1989; for a contrary view of West Germany, see Brooks 1990). On balance, this evidence supports the normative expectation, derived from democratic theory, that governments should respond to public opinion. Yet the findings of scholars who trace changes in the efficacy of representative institutions justify further research.

We explore the influence of public opinion on military spending, an issue that focuses attention on the question of whose will should predominate. Defense spending reflects a policy matter—national security—of supreme importance to all citizens, who should therefore decide how best to achieve it. Yet it also represents a realm of policy traditionally regarded as requiring some degree of expert judgment and access to privileged information. For many, it is a realm best left to those institutionally responsible for the common defense (Almond 1950; Lippmann 1955). If there were an external threat to national security that was not perceived by the public, those who govern could well be expected to act contrary to the common will. Yet policymakers might also act contrary to the common will in the absence of such a danger.

The issue of whose will should predominate is one of at least four questions that are important to democratic theory. The first question is whether the majority should rule.[1] Even in an area like defense spending, which is undeniably an issue of salience to the public, the mass public can have little influence under the strict standards of majoritarian democracy. After all, there have been absolute majorities (rather than mere pluralities) supporting changes in defense spending in only two of the past 25 years (1980 and 1981). Majoritarian democracy asserts that in the absence of a majority, the status quo should be maintained. But this would imply that over most of the past 25 years public opinion should have no effect on policy. Neither control of policy by policymakers nor control by groups with some particular interest in the size of the defense budget is a solution acceptable by the norms of democratic governance (McConnell 1966; Schattschneider 1960). We argue that due to the incremental nature of policymaking in military spending, *changes* in public opinion should influence policy.

The second question concerns the needs created by representative democracy—how to meet the need for stability and predictability of policy despite momentary shifts in popular sentiment, how to protect minority rights from majority tyranny, and how to give special weight to portions of the population who may be better informed or more intensely interested in a particular policy.[2] These needs may modify the appropriateness of reflecting majority opinion in particular instances. Few observers, however, would deny that to deserve the label *democratic*, a state's institutions should, over time, produce policy outputs substantially consistent with some indication of public opinion. If public opinion influences decision makers in competition with considerations of special importance to the decision makers, then we must ask what will be the importance of public opinion relative to these other factors? To answer this question we estimate the magnitude of the influence of public opinion on defense spending and compare this with the magnitude of autonomous government influence.

Our third question concerns the degree to which public opinion *consistently* affects policy over time. Here, we are interested in systematic effects. Democratic theory's requirements are not satisfied by merely occasional compliance with the common will.

Finally, we ask whether consistency between public opinion and policy arises because public opinion influences government or because the government influences public opinion. Sometimes leaders adopt policies that are not initially preferred by the public, and approval develops subsequently. Scholars generally agree, however, that policy adoption should not consistently precede the emergence of popular approval in a democracy. Questions have been raised by a number of scholars (e.g., Edelman 1964; Lindblom 1977; Miliband 1969; Weissberg 1976). More recently, Ginsberg has asserted that by "using polling, media and public relations techniques, modern states have learned a good deal about the manipulation and management of public opinion" (1986, 224).[3] To answer this question, we empirically test how far public opinion has consistently *responded to* changes in U.S. military spending.

AN ALTERNATIVE TO THE MAJORITY RULE

In order to assess the relative influence of public opinion, and in order to model the decision-making process, we must define how change in a policy area (military spending) occurs. For budgetary issues like military spending, change must occur in an incremental manner. We argue that for such decisions the definition of individual interests in democratic theory can remain unchanged but that the majority rule must be replaced. In general, we suggest that with each budget cycle, the government should be expected to support incremental changes in the defense budget that reflect *changes* in the level of public support or opposition to increases in military spending.

Policy decisions regarding military spending and other budgetary items are made in increments (Wildavsky 1964). Making only incremental changes in the budget eliminated certain inefficiencies in the cost of prosecuting the Cold War. Annual budgets fund multiyear projects in which substantial investments are at stake; by changing military spending at the margin, many of the costs of stopping and restarting projects, or of abandoning investments only to reinvest, were avoided.

What implications do incremental decisions have for democratic theory? The incremental politics of the military-budget-making process pose no special problem for the definition of individual interests. We follow Dahl (1989), who uses a twofold argument to justify a particular definition of individual interests that gives a prominent place to the values of inclusiveness and equality. First, "the good or interests of each person must be given equal consideration." Dahl argues that "during a process of collective decision-making, the interests of every person who is subject to the decision must (within the limits of feasibility) be accurately interpreted and made known" (pp. 86–87). Second, Dahl defends a "presumption of personal autonomy" implying that "each adult person whose interests are involved in the outcome ought to have the right to specify what those interests are" (p. 99).

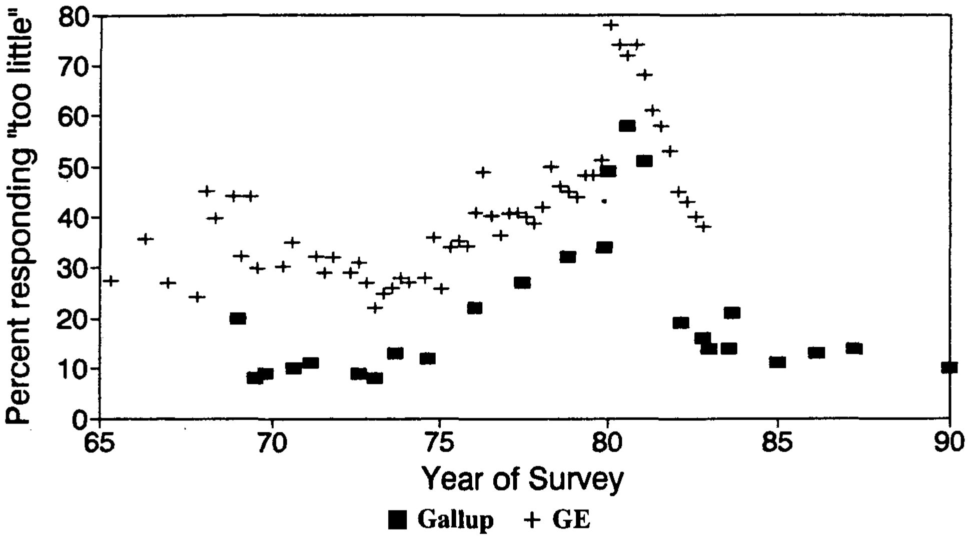
Random sample surveys satisfy both of Dahl's criteria and so can be thought to reflect the public interest accurately. In such a survey, each individual has an equal probability of voicing an opinion, and each individual's opinion is usually weighted equally.[4]

If incremental politics pose no problem for the definition of individual interests, they do pose some problems for the choice of a decision rule. When analyzing the effect of public opinion on policy in a representative democracy, democratic theorists have long used the principle of majority rule in order to justify the United States' democratic institutions of governance. Certain kinds of policies lend themselves to the majority rule—requiring a vote for enactment and a vote for repeal. The changes of incremental politics, in contrast, are small relative to the absolute size of the budget and do not constrain future action. It is reasonable to expect that in many years so many individuals whose interests are at stake might abstain from following the policy debate that no absolute majority would favor change in a particular direction.[5]

If the regular emergence of majorities is unlikely, then we must abandon the requirement of majority rule and search for a weaker rule that is consistent with the norms of democratic governance. In the United States, we often neither have nor need majority support in policy areas where decisions are characterized by incremental change. When there is no majority, we still expect the government to pay attention to changes in public opinion. When support for further increases in military spending drops, the government should respect this shifting preference in its incremental decisions by not increasing defense spending (barring temporary considerations such as a newly identified threat). Conversely, we might expect government to spend more on the military when support for increases in military spending rises. Consistent failure to do so would be to ignore the pref-

FIGURE 1

Gallup and General Electric Opinions on Change in Military Spending Compared



erences of the public most of the time. In policy areas where decisions recur periodically and change occurs incrementally, our rule satisfies the requirements of democracy better than the rule of the majority. Policymakers should be interested not only in whether a majority exists but also in the direction that public opinion is currently moving.

In short, testing governmental responsiveness in a policy area where change is incremental would involve examining change in public sentiment and change in government policy. We shall measure the importance of public opinion relative to other factors that might influence policy makers in the area of military spending, looking for a systematic influence of public opinion on policy. We will then see whether changes in public preferences for increased or decreased military spending precede governmental decisions to increase or decrease military spending. We now turn to the specification and estimation of our model.

MODELING MILITARY SPENDING

Changes in military spending are expected to be influenced by changes in public support for, and opposition to, changes in military spending. Before specifying our model, however, we must consider some issues of measurement.

We are concerned with actual expenditures rather than budgetary projections. Jacobson (1985) measures overall U.S. military spending with *total obligational authority* (TOA). We prefer total obligational authority to *budget authority* (BA) because spending is not completely determined by the final budget approved by Congress and the president. For example, some budget authority lapses before obligations are incurred, making TOA smaller than BA. An additional advantage of TOA over other indicators for military spending is that the military constructs a deflator specifically for TOA, and this deflator more accurately reflects changes in costs than does a deflator intended for use with the entire budget.[6]

Measuring public opinion also requires some discussion. One appropriate survey question used by several survey houses asks whether military spending should increase or decrease. With the problem conceptualized in terms of change, those with no current interest could either fail to express an opinion or simply support the status quo.

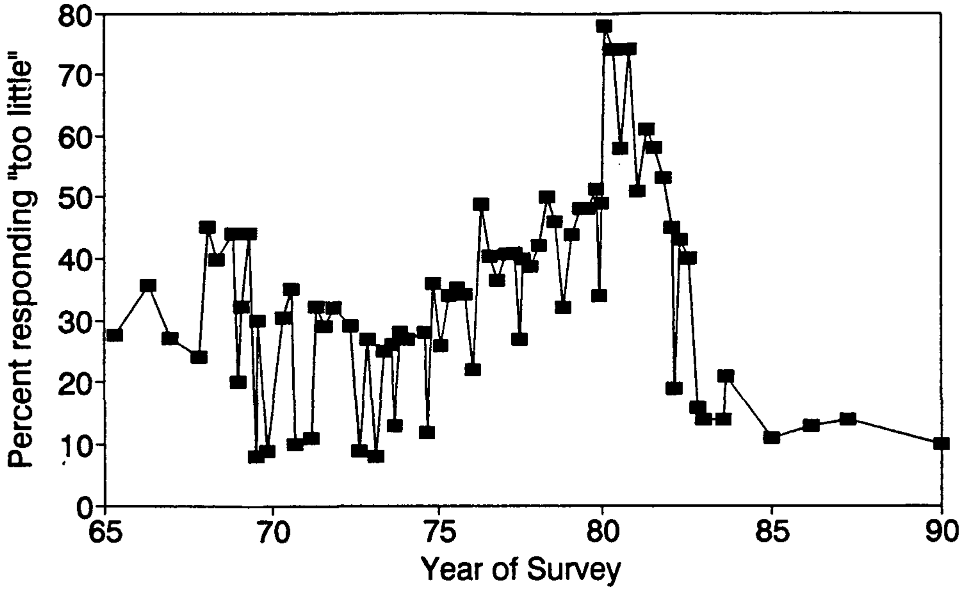
Public opinion is not measured at regular intervals, nor do any of the survey houses which measure support for changes in military spending produce enough estimates for us to be able to use a single survey house's estimates. We use a dataset compiled by one of the present authors and Thomas Graham (Graham and Hartley 1990). The dataset goes back to 1965, further than other available measures of public opinion; and it is carried forward to 1990. It allows us to avoid the methodological problems caused by a limited number of degrees of freedom and to check for the robustness of our results. Also, because we have data from a variety of sources (six different survey houses), we are able to ascertain the sensitivity of our results to the effect of house and question bias.

Using our comprehensive data set, we pooled the different public opinion series over a relatively long time series. Figure 1 illustrates that a single polling organization's measurements tend to move relatively smoothly over time. In contrast, Figure 2 shows that pooling the observations from different survey houses leads to great variance.

To ensure that survey house bias and variance do not affect our results, we construct three indicators of public opinion. The first is created by averaging

FIGURE 2

Gallup and General Electric Opinions on Change in Military Spending Combined



pooled public opinion measurements for all six survey organizations over annual periods that correspond with the budgetary cycle. Gallup, Roper and National Opinion Research Center tend to cluster together with less variance, and so were used to generate the second indicator. A third, less-dovish, indicator was obtained using data from General Electric; Columbia Broadcasting System; and Yankelovich, Skelly, and White.[7] Figures 3 and 4 illustrate how each group of three survey houses produces similar trends. Regressions were run using the indices derived from all three of these pooled indicators. Some results are not reported here, because the use of different groupings did not affect our findings. The coefficients from each of the three regressions were statistically significant and their slopes led to the same substantive conclusions. The series we use to generate the reported estimates is from all six survey houses averaged annually and can be found in Table 1.[8]

So far we have only discussed our dependent variable, U.S. military spending, and one independent variable, public opinion. Several other important factors are needed to specify the model. Policy might also be expected to reflect the concerns of officials and specialists faced with the conflicting

FIGURE 3

Hawkish Bias: General Electric; CBS; and Yankelovich, Skelly, and White Opinions on Change in Military Spending

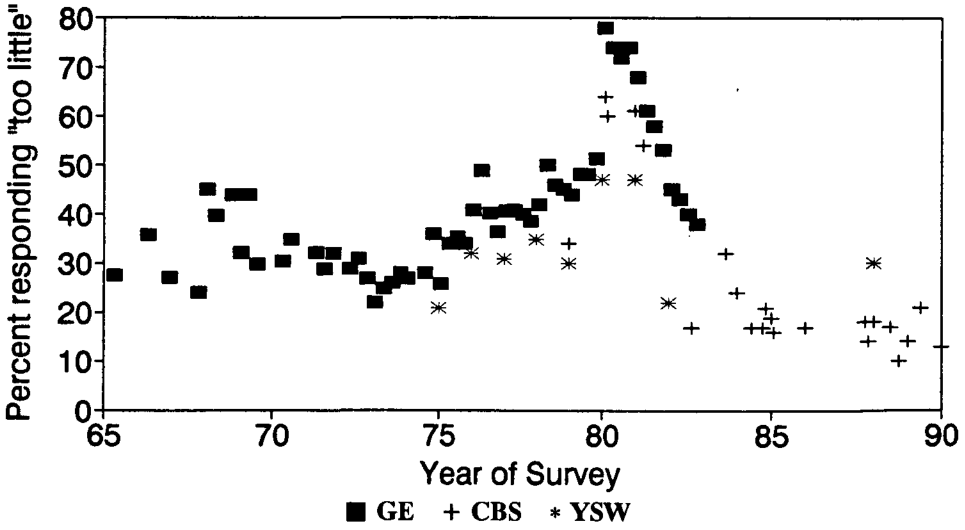
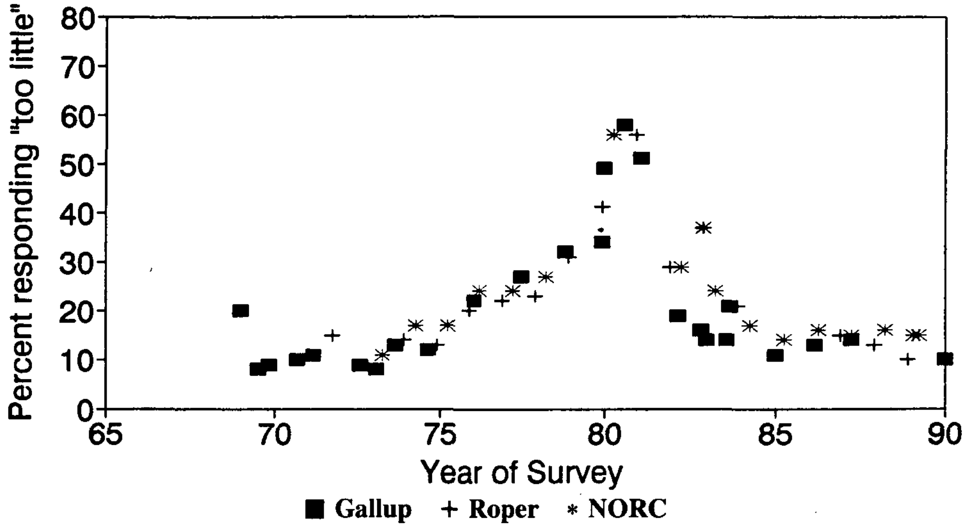


FIGURE 4

Dovish Bias: Gallup; Roper; Yankelovich, Skelly, and White Opinions on Change in Military Spending



imperatives of national security and domestic policy. A model explaining change in total military spending could draw on a variety of earlier models. (See Anderton 1989; Etcheson 1989; McGinnis 1991; Russett 1983 for reviews.) A reasonable model, that is, one in which omitted substantively significant variables are uncorrelated with our included independent variables, would include three other factors: changes in Soviet military spending, changes in the difference between U.S. and Soviet military spending, and changes in the U.S. federal budget deficit. Other variables might be added, such as bureaucratic inertia or the importance of stockpiles of armaments. However, given our goal of estimating the relative influence of the mass public on policy, these variables, plus a term for Soviet conflictual or cooperative behavior toward the U.S., should suffice.[9]

TABLE 1

Averaged Values of Survey Responses Concerning Military Spending, 1965–1990 (%)

YEAR	"TOO LITTLE"	"TOO MUCH"
1965	28	16
1966	36	13
1967	27	22
1968	36	14
1969	35	24
1970	19	36
1971	22	37
1972	26	28
1973	20	32
1974	21	30
1975	23	30
1976	32	21
1977	33	18
1978	37	16
1979	40	16
1980	58	11
1981	60	10
1982	35	25
1983	22	36
1984	21	30
1985	17	31
1986	15	37
1987	16	39
1988	17	33
1989	14	34
1990	12	43

Sources: See Appendix A.

Our first policy-related variable, Soviet military spending, reflects the assumption that if military spending could have been affected by public opinion, it could also have been spurred by an arms race. Arms race models have emphasized international causes—adversary's military spending or military strength, international tensions, and the like (Richardson 1960; Ward 1984). Most arms race models assume that military spending is an important measure of a nation's future military capabilities and therefore also intentions and commitment. Marra and Ostrom (n.d.) and Ward and Davis (1990) argue that the measure of aggregate Soviet military spending reflecting the information most likely used by U.S. policymakers *at the time their budgetary decisions were made* is generated from the "building block method" used by the Central Intelligence Agency, and published by the U.S. Arms Control and Disarmament Agency.[10] This method involves estimating the change in tangible assets of the Soviet military (e.g., tanks, aircraft) and then estimating what it would cost to duplicate this capability in the United States. Marra and Ostrom argue that the most relevant features of Soviet military spending for U.S. policymakers are the sign and size of annual changes. They also argue for the importance of the gap between U.S.

and Soviet military expenditures. We use the constant dollar estimate for U.S. and Soviet military spending because policymakers try to account for inflation. Inflation-adjusted budgetary outlays also reflect the real economic resources that would be required to produce, maintain, and use a given level of military resources and that are ultimately paid for by the public.[11]

If changes in U.S. military spending are the product of policymakers' national security considerations, they are also determined in part by the administration and Congress who must consider domestic political influences, the most important of which are fiscal constraints. Whereas the armed forces might be expected to support expenditures that increase military capability in response to the Soviet Union, the administration and Congress need to balance increased military expenditures with domestic priorities such as an increasing budget deficit (Huntington 1961, 125; Ostrom and Job 1986).[12]

We estimated two equations. Both of these equations are differenced, so the constant captures any linear trend in U.S. military spending. The first one includes the policy variables and an indicator of the extent to which public opinion feels the government is spending too little on defense:

change in total U.S. military spending = constant

+ change in Soviet military spending$_{t-2,\,t-3}$

+ change in (Soviet military spending

− U.S. military spending)$_{t-3}$

− change in deficit$_{t-1}$

+ change in percentage responding *too little*$_{t-1}$.

The second equation is the same as the first except that it reads *too much*, instead of *too little*. We expect a negative coefficient for this variable.

A note is in order regarding lag lengths. Marra and Ostrom (n.d.) and others have argued that a lag length of three years is required for measures that include Soviet military spending: one year because budgets are presented and debated in the year preceding the one they cover and two additional years to allow for the time required to turn evidence of Soviet military strength into Arms Control and Disarmament Agency (ACDA) estimates of Soviet military spending. Since information on Soviet military spending is not available in the first year after the Soviets make their budget, we do not include the earlier lag. A one-year lag for Soviet military spending would have no theoretically meaningful interpretation. However, we do include a lag length of just two years to test for the possibility that preliminary and confidential estimates are used in the budgetary process.

Note also that we did not include a lagged dependent variable, because we are dealing with differences in U.S. military spending. While last year's level might be expected to be a good predictor of this year's level, we would expect this year's changes to result from variables important to policymakers, such as the latest estimate of the Soviet threat and public opinion, not from last year's change.

TABLE 2

Explaining Changes in U.S. Military Spending, 1965–1990

INDEPENDENT VARIABLES	COEFF.	S.E.	*P*
Constant	−1.29	5.37	—
Change in			
Soviet military spending$_{t-2}$	.357	.257	.08
Soviet military spending$_{t-3}$	.503	.173	.01
Difference between Soviet and U.S. military spending$_{t-3}$	−.151	.173	.18
Deficit$_{t-1}$	−.064	.028	.02
"Too Little"$_{t-1}$	.261	.149	.05

Note: $\bar{R}^2$ = .77; standard error for the equation = 5.44; degrees of freedom = 14. All monetary units are billions of constant (1982) dollars. P-values are for one-tailed t-tests.

FINDINGS

Table 2 shows our results.[13] The variable of interest here is the public opinion variable. We find evidence that increases in the percentage of the public that believes that the government is spending too little on the military result in increases in military spending ($b = .261, p < .05$). The statistical significance level for this independent variable is impressive, considering that the indicator is the average of values taken from different survey houses.[14] Substantively similar results hold if we use as our independent variable the percentage of the public responding that the government spends too much on military spending.

The greatest influence on change in U.S. military spending is change in Soviet military spending. Change in Soviet military spending as reported by the ACDA, lagged three years, is substantively and statistically significant ($b = .503, p < .04$). We find support for Ostrom and Marra's (1986) assertion that decision makers primarily use publicly available information regarding Soviet military spending. Change in Soviet military spending, lagged only two years, has a marginally statistically significant influence on change in U.S. military spending ($b = .357$, $p < .08$). Because these variables covary, it is likely that Soviet military spending is even more statistically significant than our results indicate.

We do not find statistically significant results for the gap between U.S. and Soviet spending ($p < .18$). It would seem that in the political debate the gap does not add much, but, rather, U.S. spending responds to Soviet spending. The sign of the coefficient for the deficit is in the expected direction, with increased deficits exerting pressure against further increases in U.S. military spending ($b = -.064$, $p < .02$).

Two other variables that were not included in our model nevertheless merit special attention. Models of arms races and of Soviet-American relations suggest that military expenditures are affected by changes in the level of international political tensions, that is, by the behavior, as well as the expenditures, of the adversary. To account for the influence of international tension, we included the indicator of Soviet behavior toward the United States constructed by Joshua Goldstein from the World Event/Interaction Survey dataset aggregated to the annual level (Goldstein and Freeman 1990).[15] However, we found no significant effect.

We also might expect military spending to be greater during the Reagan years, all else being equal. Analysis (not reported in the table) using dummy variables for Reagan's first term, (1981–84), the years 1981–88, and the years 1981–90 did not produce stable and statistically significant coefficients. It appears that increases in military spending during those years are explained better by changes in the other variables, including public opinion.

Our results also suggest that the effect of public opinion on policy is consistent. In fact, regressions run with either earlier years or later years omitted from the sample resulted in the same conclusions.

In short, much of the influence on military spending stems from "objective" characteristics of the arms race, such as the change in Soviet military spending. Some of these influences on military spending other than public opinion can be considered normatively justifiable. One might argue with the wisdom of engaging in an arms race, but it would be difficult to refuse experts some autonomy to assess the external military threat and react to it. Other nondemocratic influences would have been more controversial; but there is no strong evidence that a significant part of the increase in military spending during the Reagan years was due to the particular perspectives of his administration, independent of other factors.

Even controlling for the effects of other factors that concern policymakers, we find that public opinion exerts an effect on military spending in a way that is substantively plausible as a cause. With other influences held constant, we find that if the percentage of public opinion favoring increases in military spending rises, then the level of military spending will increase. Conversely, if the percentage of public opinion opposing increases in military spending rises, then actual spending tends to come down. It is important to note that these effects were systematic over the time period that we studied, not occasional effects too erratic to result in a statistically significant regression coefficient.

More specifically, ceteris paribus, for every 1% change in support for (or opposition to) increased spending there is about a $.33 billion increase (or decrease) in defense spending. This is a small but not insignificant effect, especially considering that swings in public opinion were as large as 25% during the late 1970s and early 1980s (see Figure 1). Neither, however, does the public have an overwhelming impact on the nearly $300 billion military budget.

ASSESSING THE INFLUENCE OF GOVERNMENT ON PUBLIC OPINION

We shall test the hypothesis that consistency arises because the government influences public support for increases (or decreases) in military spending. When it changes the military budget, does the government create a change in public opinion supporting that action? To address this question partially, we use Granger's concept of causality to assess the directionality of influence. Granger causality—developed by Granger (1969) and used in vector autoregression (Freeman, Williams, and Lin 1989; Sims 1980)—is a method that relaxes assumptions about causal direction and lag length and so allows one to assess the directionality of influence. The technique involves regressing each variable on a reduced form equation that includes its own lagged values and the other variables' current and lagged values. We hypothesize that government does not influence public opinion. In the language of Granger causality, we hypothesize that changes in policy do not "Granger-cause" changes in public opinion, that is, that lagged values of public opinion would do just as well explaining current values of public opinion as would a model that also included lagged values of military spending.[16]

One potential criticism of our use of vector autoregression is discussed in Appendix B. It can be avoided by making the reasonable assumption that there is a first-order autoregressive process in U.S. military spending. The finding of autocorrelation in our regressions supports this assumption.

Our model includes three lags each of public opinion, U.S. military spending, and Soviet military spending. We tested the hypothesis that change in U.S. military spending Granger-causes subsequent change in public support for military spending in two ways—that $\pi_{12} = 0$ first for public support for increased, and then for decreased, military spending. The results were far too weak to be acceptable at any reasonable significance level (chi-squared [3] = 1.87, $p = .60$; chi-squared [3] = 2.66, $p = .44$).[17] The absence of significant results in either equation suggests that public opinion may in fact be exogenous to policy on military spending.[18]

CONCLUSIONS

We have assessed how far public opinion influences government policy in the realm of military spending. Despite other researchers' findings that institutions that maintain public control over government may be losing their efficacy, we found strong evidence consistent with the hypothesis that public opinion did in fact influence government policy, although the exigencies of the arms race and the budget deficit were equally or more influential. The influence of public opinion was systematic over a period of years. One might regard this mixture of public and elite influence as acceptable, at least in an era when the Soviet Union was actively building up its military and a countervailing military buildup might be necessary. We also found some evidence supporting the contention that public opinion was not merely the result of overt government influence.

A great deal of research remains to be done in this area, however. For one thing, it is uncertain whether public opinion affects policy consistently in other areas of incremental politics, and it is uncertain to what degree public opinion will continue to influence military spending in the absence of a superpower arms race. Second, it is uncertain what influence the media has on the process. It is possible that network television (as documented in the Vanderbilt Archives) has an effect on public opinion but no direct influence on policy. Network media might thus be used as an instrument to specify a simultaneous equation model to test further the hypothesis that government influences the public (see Page, Shapiro, and Dempsey 1987). Third, arms race modelers might consider the effect of public opinion in models in which U.S. and Soviet budgets affect each other simultaneously and in which military strength is also a factor; that might provide a more accurate measure of the relative importance of public opinion and arms race coefficients. Fourth, the findings of Ostrom and Marra (1986) suggest the possibility that different branches of government are more responsive than others in this policy area. Analyzing each of these three questions would require several more years of data if results are to be subjected to sensitivity analysis. Fifth, it is uncertain what the structure of public opinion that underlies the changes in preferences on military spending is. Analyzing this would require panel data. Finally, all of these results are merely suggestive. To strengthen an assessment of causality, historical research is required, to show that policymakers actually sought out and used information regarding public opinion when making their decisions.

APPENDIX A: DATA SOURCES FOR PUBLIC OPINION INDEX

All of the public opinion data used in this analysis can be found in Graham and Hartley 1990. Question wording for the different survey houses is listed below.

Gallup, American Institute of Public Opinion, Gallup/*Newsweek*, Gallup/*Times Mirror*

Version 1. "There is much discussion as to the amount of money the government in Washington should spend for national defense and military purposes. How do you feel about this: Do you think we are spending too little, too much, or about the right amount?"

Version 2. "Do you think that we should expand our spending on national defense, keep it about the same, or cut back?"

The Roper Organization

"Turning now to the business of the country—we are faced with many problems in this country, none of which can be solved easily or inexpensively. I'm going to name some of these problems, and for each one I'd like you to tell me whether you think we're spending too much money on it, too little money, or about the right amount. Are we spending too much, too little, or about the right amount on . . . the military, armaments and defense?"

National Opinion Research Center

"We are faced with many problems in this country, none of which can be solved easily or inexpensively. I'm going to name some of these problems, and for each one I'd like you to tell me whether you think we're spending too much money, too little, or about the right amount: . . . military, armaments, defense."

General Electric/Trendex

Version 1. "I would like to get your opinion on several areas of important expenditures, first on the part of the government. As I read each one, please tell me if you would like to see the government spend more, spend less, or spend about the same amount as they have been . . . on defense."

Version 2. "I would like to get your opinion on several areas of important government activities. As I read each one, please tell me if you would like to see the government do more, do less, or do about the same amount as they have been: . . . defense."

CBS, CBS/*New York Times*, *New York Times*

Version 1. "Should federal spending on military and defense programs be increased, decreased, or kept about the same?"

Version 2. "Do you think government/federal spending on military and defense programs should be increased, decreased, or kept about the same?"

Yankelovich, Skelly, and White/*Time*; Yankelovich, Clancey, Shulman/*Time*

"Do you feel that government spending should be increased, decreased, or kept where it is now in the following areas? . . . the military."

APPENDIX B: CRITICISM OF VECTOR AUTOREGRESSION

One criticism of vector autoregression is important to our example (Cooley and LeRoy 1985). Note that our model is

$$\begin{bmatrix} X_1 \\ X_2 \\ X_3 \end{bmatrix} = \begin{bmatrix} 0 & \alpha_{12} & \alpha_{13} \\ \alpha_{21} & 0 & \alpha_{23} \\ \alpha_{31} & \alpha_{32} & 0 \end{bmatrix} \begin{bmatrix} X_1 \\ X_2 \\ X_3 \end{bmatrix} + \begin{bmatrix} A_{11}(L) & A_{12}(L) & A_{13}(L) \\ A_{21}(L) & A_{22}(L) & A_{23}(L) \\ A_{31}(L) & A_{32}(L) & A_{33}(L) \end{bmatrix} \begin{bmatrix} X_1 \\ X_2 \\ X_3 \end{bmatrix} + \begin{bmatrix} u_1 \\ u_2 \\ u_3 \end{bmatrix},$$

where X_1 = public opinion, X_2 = U.S. military spending, X_3 = Soviet military spending, and L is the lag operator, so that $A_{11}\ (L^1)X_1 = A_{11}X_{1\ t-1}$ and $A_{11}\ (L^2)X_1 = A_{11}X_{1\ t-2}$. The reduced form of this model is

$$\begin{bmatrix} X_1 \\ X_2 \\ X_3 \end{bmatrix} = \begin{bmatrix} \pi_{11}(L) & \pi_{12}(L) & \pi_{13}(L) \\ \pi_{21}(L) & \pi_{22}(L) & \pi_{23}(L) \\ \pi_{31}(L) & \pi_{32}(L) & \pi_{33}(L) \end{bmatrix} \begin{bmatrix} X_1 \\ X_2 \\ X_3 \end{bmatrix} + \begin{bmatrix} v_1 \\ v_2 \\ v_3 \end{bmatrix}.$$

We can accept the null hypothesis of no Granger causality if the parameter π_{12} does not differ significantly from 0. Military spending (or the influence of government) is said not to Granger-cause public opinion if $\pi_{12} = 0$. However, Cooley and LeRoy (1985) point out that Granger causality is distinct from strict exogeneity. Cooley and LeRoy would argue, in this case, that public opinion can be considered to be causally prior to military spending if and only if public opinion is strictly exogenous and military spending is endogenous. In this case, public opinion is strictly exogenous with respect to U.S. military spending if $\alpha_{12} = A_{12}(L) = 0$. Note that the reduced form equation is

$$\begin{bmatrix} X_1 \\ X_2 \\ X_3 \end{bmatrix} = \frac{1}{|\alpha|} \begin{bmatrix} 1 - \alpha_{23}\alpha_{32} & \alpha_{12} + \alpha_{13}\alpha_{32} & \alpha_{12}\alpha_{23} + \alpha_{13} \\ \alpha_{21} + \alpha_{23}\alpha_{31} & 1 - \alpha_{13}\alpha_{31} & \alpha_{23} + \alpha_{13}\alpha_{21} \\ \alpha_{21}\alpha_{32} + \alpha_{31} & \alpha_{32} + \alpha_{12}\alpha_{31} & 1 - \alpha_{12}\alpha_{21} \end{bmatrix} \begin{bmatrix} A_{11}(L) & A_{12}(L) & A_{13}(L) \\ A_{21}(L) & A_{22}(L) & A_{23}(L) \\ A_{31}(L) & A_{31}(L) & A_{33}(L) \end{bmatrix} \begin{bmatrix} X_1 \\ X_2 \\ X_3 \end{bmatrix} + \begin{bmatrix} v_1 \\ v_2 \\ v_3 \end{bmatrix},$$

where $|\alpha| = 1 - \alpha_{23}\alpha_{32} - \alpha_{12}\alpha_{21} - \alpha_{12}\alpha_{23}\alpha_{31} - \alpha_{13}\alpha_{21}\alpha_{32} - \alpha_{13}\alpha_{31}$, so that

$$\pi_{12} = \frac{(1 - \alpha_{23}\alpha_{32})A_{12}(L) + (\alpha_{12} + \alpha_{13}\alpha_{32})A_{22}(L) + (\alpha_{12}\alpha_{23} + \alpha_{13})A_{32}(L)}{1 - \alpha_{23}\alpha_{32} - \alpha_{12}\alpha_{21} - \alpha_{12}\alpha_{23}\alpha_{31} - \alpha_{31}\alpha_{21}\alpha_{32} - \alpha_{13}\alpha_{31}}.$$

There are several circumstances in which public opinion would not be strictly exogenous and yet π_{12} would equal 0, leading to a false finding of exogeneity. One of these circumstances is relatively unlikely, namely, that in which $1 = \alpha_{23}\alpha_{32}$, $\alpha_{12} = -\alpha_{13}\alpha_{32}$, and $\alpha_{13} = -\alpha_{12}\alpha_{23}$. However, π_{12} could equal zero and yet α_{12} not equal 0 if $A_{22}(L) = A_{32}(L) = 0$ and either $A_{12}(L) = 0$ or $1 = \alpha_{23}\alpha_{32}$. We can avoid this pitfall by assuming that $A_{22}(L)$ does not equal 0, that is, by assuming that there is a first-order autoregressive process in the military spending budget. This is a theoretically justifiable assumption and is empirically supported by our finding resulting from the correction of first-order autocorrelation reported in the results.

Notes

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1. Few observers in modern states would make the requirement for a simple majority absolute. For one thing, it is well known that if policy issues are distributed on multiple dimensions, various shifting majorities may form even under procedures widely regarded as democratic (Arrow 1963; Caplin and Nalebuff 1988, 1991).

2. See the *Federalist Papers* 10.

3. Some evidence indicates that crises and other factors are used to raise the popularity of leaders when the economy is weak or elections are near (Brody 1984; Kernell 1978; Mueller 1973; Ostrom and Job 1986; Russett 1990, chap. 2; Russett and Barzilai n.d.).

4. Actual sampling procedures may, of course, deviate from the assumptions without generating a biased sample.

5. An additional reason why a majority might not emerge in the realm of military spending is that issues of national defense are complex. They entail balancing the risk of international conflict with domestic priorities. It is never clear "how much defense is enough" or whether additional military spending serves as a deterrent or as an incentive to arms race or war. There is never an unambiguous answer to the question of whether to increase or decrease military spending. Many individuals select the answer *don't know*.

6. Data on U.S. military spending come from U.S. Department of Defense 1990.

7. For a more detailed discussion of house bias in aggregate survey data, see Cowden and Hartley 1991. Question wording is included in Appendix A, but it is important to note that question wording does not seem to be the only determinant of bias in aggregate public opinion measurements.

8. Note that we cannot include each survey house individually because no single house measured public opinion on military spending at regular intervals over the period. Surveys dating further back are available, but it is impossible to assess their comparability because there is no overlap in the early 1960s. We could have extended the analysis into 1991, on the basis of public opinion in 1990. But we preferred to restrict the analysis to the Cold War era, during which we would expect the coefficients to be reasonably stable. In both November 1989 and January 1990, when the public was asked, "In view of recent events in the Soviet Union and Eastern Europe, do you think the cold war is over?, only 37% said *yes* and 54% said *no*. But by October 1990, following German unification, 49% said *yes* and 43% *no* (CBS/*New York Times* press releases).

9. Marra and Ostrom (n.d.; Ostrom and Marra 1986) have discussed the specification of a model explaining what policymakers consider in determining levels of U.S. military spending. Since we are interested in assessing the overall responsiveness of American government to public opinion, we do not separately model the different institutions that participate in the budgetary process. Two variables that they considered and we excluded are change in war costs and change in revenues. The Vietnam War was primarily deficit-financed; thus, war spending should be primarily correlated with the deficit. Excluding war costs might be expected to attenuate the deficit coefficient, because its sign will be

negative, the coefficient for the war cost variable would be positive, and the two independent variables should be positively correlated. Change in revenue is probably not correlated with any of the independent variables, so that its exclusion should not bias any of the estimates.

10. This estimate is reported annually in the U.S. Arms Control and Disarmament Agency's *World Military Expenditures and Arms Transfers*. The Defense Intelligence Agency estimate was also very probably used by policymakers, but it is not in the public domain. It is less likely that alternative estimates, produced by Stockholm International Peace Research Institute and International Institute of Strategic Studies, were used by U.S. policymakers.

11. For additional arguments regarding the consideration of military spending in constant dollars, see Kinsella 1990. Kinsella also argues persuasively that there is no evidence that military spending has a negative effect on the economy at the macroeconomic level. Thus, other measures of fatigue (e.g., Richardson 1960) are not applicable in this context:

12. Data for our measure of the deficit was taken from U.S. Office of Management and Budget (1991) for fiscal year 1989.

13. First-order autocorrelation was corrected for using the Cochrane–Orcutt method; rho was .408; higher-order autocorrelation was not found.

14. It is important to be cautious regarding the interpretation of the magnitude of the coefficients for the public opinion variables. Separating out hawkish from dovish survey houses (as in Figure 2) changes the magnitude of this variable's coefficient, though not its sign or statistical significance. However, since this variable is almost certainly measured with random error, all coefficients obtained are conservative estimates.

15. Goldstein and Freeman's data end in June 1990; we have used the first six months of 1990 as an estimate for the entire year. They caution that the coder changed in 1986 and 1987.

16. It is worth noting at this point that to be exact, our theoretical question is not, To what does extent actual military spending influence public opinion? Rather, we are using U.S. military spending as a proxy and asking, Do the officials who influence the complex process of policymaking in military spending also influence public opinion?

17. Since there is no statistically significant finding, we do not discuss the magnitudes of the response here. Full results of our analysis, including impulse response functions and innovation accounting, are available on request.

18. Eichenberg and Stoll (1991) suggest that, if anything, a change in military spending subsequently produces an opposite change in public preferences, as the public reacts against perceived policy extremes. They do not, however, use vector autoregression or a simultaneous equation model.

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PATTERNS AND SOURCES OF TICKET SPLITTING IN SUBPRESIDENTIAL VOTING

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The primary source of divided government in the United States is voters who split their ballots between the parties. Yet there has been little comprehensive examination of either patterns or sources of ticket splitting in recent years. Instead, divergent lines of research have emerged, emphasizing such things as voter partisanship, incumbency, and a "new" (young, well-educated, even partisan) kind of ticket splitter; and their focus has been too often restricted to the atypical president–Congress pair. We seek to unify these research traditions in a comprehensive model of split-ticket voting and to test this model across the partisan ballot in a typical election setting–here, the contests for five Ohio state-wide offices in 1990. The model incorporates partisan strength, candidate visibility, and the individual characteristics that distinguish the "new ticket splitters". The results support our partisan strength and candidate visibility explanations but provide little support for the emergence of a new type of ticket splitter.

Divided control of government, that uniquely American form of coalition government in which two different parties simultaneously control key governmental institutions, may be the most important political phenomenon of our time (Cox and Kernell 1991; Fiorina 1990; Jacobson 1991; Mayhew 1991; Sundquist 1988). In 20 of the last 24—and 32 of the last 48—years an American president has faced a Congress in which at least one of the houses was controlled by the opposition party. In the earlier years of American national politics, by contrast, divided control emerged from time to time, usually as a result of midterm election reversals for the presidential party; but it never dominated as it has in recent years.

Divided control is hardly exclusive to the banks of the Potomac. As V. O. Key (1956) noted 35 years ago, it has long been common in the state governments; and it has become increasingly widespread in recent years. The 1990 elections produced 32 states[1] with a governor of one party and a legislature in which the opposition party controlled at least one house—tying the historical high in 1988 but considerably higher than the average since 1967 or for the 1931–52 period (Fiorina 1990; Key 1956, 55).

The constitutions of almost all states provide a further possibility for divided government by separately electing different executive branch officials. In 1991, 35 states had top officeholders from both parties. This figure is well above the average over the last 20 years, and recent levels of executive splits greatly exceed those reported by Key for selected states during the 1900–1952 period (Key 1956, 197–216). When party control of the plural executive is combined with control of the legislature, only 12 states (most of them in the South) emerged from the 1990 elections as wholly dominated by a single party. Since 1967, in fact, there has never been a year when a majority of the states had single-party control of all executive offices and the state legislature. Divided control, in short, has become the *norm* for state governments just as it has for the government in Washington. Its causes, therefore, are a worthy topic of investigation.

Scholars have offered numerous structural reasons for divided government (Fiorina 1990; Jacobson 1991; Key 1956). Separate elections for different officials and the staggering of elections for various offices always have provided opportunities for divided party control in America that are not available in most democratic nations. The translation of popular votes into seats (or into electoral votes in the presidential contests), including the effects of gerrymandering, also may produce divergent results for different offices from the same vote totals. The office-block ballot form adopted in some of the states is conducive to divided government as well, with the arrangement of candidates by office (rather than party) and the correlative absence of a single-action party-vote choice making straight-party voting more difficult.

The principal roots of divided government are to be found in voter choice, however, not political structure. The direct election of multiple officials—a legacy of the powerful democratic tide in the Jacksonian era—*permitted* a voter to support different parties on the same ballot once the consolidated Australian ballot became widespread after the 1890s.[2] The office-block form, a legacy of the Progressive Era, virtually extends this permissiveness to license. But voters must be motivated to split a ticket in order for these possibilities to become realities.

More and more voters in recent decades have been seizing the opportunities to split their votes between the parties in the same elections. Based on reported votes for president and the U.S. House of Representatives in the American National Election Study surveys, split-ticket voting rose from 13% in 1952 to a historic high of 30% in 1972 and has settled into the

25%–28% range since then (Stanley and Niemi 1990, 132; Wattenberg 1986, 17–23). That ticket splitting has exhibited parallel gains in other office combinations can be inferred from the increasing frequency of split results. At the individual level, then, contemporary American voters are less motivated than before—probably than ever before—to vote only for the candidates from a single party.

Who are these ticket splitters? Past studies of ticket splitting have relied heavily upon vote divergence in a pair of contests—typically, the presidential and congressional races—to address this question. In an electoral system in which voters can vote for a multiplicity of offices, however, such a two-contest focus surely underestimates the extent of ticket splitting. Moreover, by restricting attention largely to the presidential and congressional contests, researchers have selected a pair that is quite unrepresentative of the situation individual voters typically face. The presidency is the most visible office in the land, and virtually all voters possess considerable information about both parties' candidates for it. Congressional contests, by contrast, typically involve a relatively visible incumbent running against an almost invisible challenger, if any. These visibility differences may accentuate ticket splitting. Furthermore, voters have very different expectations about candidates for the two offices—the one executive, the other legislative. This may translate into quite dissimilar criteria for choice (Fiorina 1990) and higher levels of ticket splitting for this pair than for offices that are similar in function. These considerations combine to make the presidential–congressional pair unique.

This study examines ticket splitting from a new and different vantage point. It focuses upon voter behavior across a five-office partisan ballot, rather than for a particular pair of offices. The case that we examine is the 1990 statewide election in Ohio. The five partisan races appearing on the ballot in that year provided a nice mix of the two characteristics—candidate visibility and the presence of an incumbent—that have proven important in earlier studies of ticket splitting. They also produced victories by candidates from each party and by both incumbents and challengers.[3]

Substantively, this focus moves us away from the current overdependence of research on ticket splitting that involves votes for president (an atypical office in terms of the information voters possess about its candidates) and toward the more commonplace low-information contests. It also examines this behavior in voting for offices at the state level. States are important policy actors in the American federal system, but they largely have been ignored in studies of voting behavior. To establish the generality of ticket splitting and its determinants requires a focus on the states.

There are good methodological reasons for this focus as well. First, we can examine electoral choices across contests for offices in the same electorate. Studies that mix the levels of contests and electorates, such as the familiar presidential–congressional comparisons, confound individual- and contest-specific factors because members of their samples face many different congressional contests. Second, by containing our comparisons within executive contests, we avoid the confounding effects of measuring ticket splitting across offices with vastly different functions and popular expectations. Consequently, results from a single state can greatly advance our understanding of ticket-splitting behavior and can also establish a baseline for replicative efforts in other states and other circumstances.

Voter behavior in the 1990 Ohio elections was tracked with a statewide panel study of Ohio voting-age adults conducted by telephone by professional interviewers in the Polimetrics Laboratory at Ohio State University. This analysis focuses on the 567 *voters* (out of 1,033 respondents) who completed both pre- and postelection interviews. Virtually all of the variables we analyze come from wave 3 of the study, but a few are drawn from wave 1.[4]

THE SOURCES OF TICKET SPLITTING

Split-ticket voting is hardly a new phenomenon in American electoral politics. Its trace markings in split election results have long intrigued scholars (Cummings 1966, chap. 2); and since the first individual-level data on voting appeared, considerable effort has been directed to explaining why voters would choose candidates from different parties. Nonetheless, for a variety of reasons, no comprehensive theory of ticket splitting has emerged that incorporates the different explanations provided for the phenomenon.

Previous explanations of ticket splitting have followed two divergent paths. Some scholars have emphasized the role of individual characteristics (particularly, partisan loyalties) in ticket splitting. Others, especially the most recent studies, have concentrated on the nature of the candidates, particularly the visibility edge enjoyed by incumbent officeholders. Our analysis draws upon each of these research traditions.

The earliest research on ticket splitting based on surveys emphasized, not surprisingly, the distinctive characteristics of the ticket splitters. In their seminal study of ticket splitting, using the 1952 and 1956 Michigan election studies,[5] Campbell and Miller (1957) attribute split-party votes for president/senator, president/representative, and state and local offices primarily to the absence of a strong party identification to motivate a straight-party vote, particularly in states that did not enable straight-ticket voting by a single action. Further analysis of multiple contests using the Michigan surveys through 1976 (by which time ticket splitting had grown to considerable proportions) corroborates the importance of strong party identification as an inhibitor of ticket splitting (Maddox and Nimmo 1981).

DeVries and Tarrance (1972) have dissented from the prevailing view that varying levels of partisanship are the dominant individual-level force behind split-ticket voting. Their study of votes across several different offices based on national polls and state polls in Michigan and Texas concludes that the partisan-versus-independent distinction was overemphasized. Only about half of the ticket splitters they uncovered were self-identified independents, which left room for considerable ticket splitting among partisans. Two factors, however, weaken the force of their dissent. First, by ignoring the differences in intensity of partisanship among the large majority of the electorate who have party loyalties, they have understated the influence of partisanship on ticket splitting. Second, partisanship still emerges from their study as the most important correlate of ticket splitting.

The foregoing studies identified other voter characteristics that should be considered in any comprehensive explanation of ticket-splitting behavior. The lack of consistency in the correlates of these characteristics across studies suggests, however, that their exact role is questionable. Campbell and Miller (1957) found ticket splitting to be more widespread among men, the less educated, urban dwellers, and southerners. DeVries and Tarrance (1972) found it greater among those who are better educated, young, more affluent, of higher occupational status, and more dependent upon the media for their political information and touted these as the characteristics of the "new" ticket splitters. Maddox and Nimmo (1981) also showed ticket splitters to be more media-oriented and found that higher degrees of interest in the campaign induced more ticket splitting.

The three studies that examine the individual voter characteristics of ticket splitters share a focus on multiple offices in their operationalizations of ticket splitting, but their differences in other respects may account for the contrasts in their findings (Campbell and Miller 1957; DeVries and Tarrance 1972; Maddox and Nimmo 1981). The early Campbell and Miller (1957) study and the national survey results used as a part of the DeVries and Tarrance study measure ticket splitting by answers to a general question asking about the extent of ticket splitting, rather than by comparing reported votes for different offices. It is likely that these responses are skewed toward a more divided vote than actually occurred because of the social desirability of ticket splitting in American life (Feigert 1979). These studies also differ in the years they cover. Systematic explanation may have been confounded by a change in the patterns of ticket splitting as it became more prevalent after the mid-1960s.

An alternative stream of research has concentrated on the inducements to ticket splitting presented by popular and visible incumbents from the opposition party. This research has focused on presidential–congressional splits and has taken advantage of the different pairings of candidates across the various congressional districts contained in national surveys (e.g., well over 100 districts are covered in the Michigan surveys). The advantages of incumbency in a time of reduced partisan loyalties have been commonly cited as leading to the split results in presidential and congressional voting (Cover 1977). Gary Jacobson (1989, 1991) has demonstrated, however, that incumbency alone is not the key to a divided vote; rather, the failure of experienced challengers to emerge to face incumbents leaves voters with little real choice but to support the current officeholder. In short, he contends, candidate visibility, rather than incumbency per se, accounts for ticket splitting.

These alternative partisan and candidate theories of ticket splitting are not logically contradictory. The frequency of straight-ticket voting may increase generally with the strength of partisanship even while the presence of strong candidates from the opposing party may undermine party fidelity. The growth of ticket splitting in recent years may have resulted from the decline in the strength of partisan loyalties within the electorate (Wattenberg 1986), as well as from changes in the visibility of challengers. Any comprehensive theory of ticket splitting, consequently, should encompass the characteristics of voters as well as of contestants for office—especially the partisanship of the voter and the relative visibility of the candidates from whom they must choose. A comprehensive theory of this phenomenon requires the incorporation of other factors too; media habits, interest, attentiveness, education, and age may affect ticket splitting independently of partisanship and the nature of the candidates. Only by considering all of these factors together with partisanship and candidates can the divergent results of previous studies be reconciled.

TICKET SPLITTING ACROSS THE BALLOT

Data from the 1990 Ohio statewide election provide a highly appropriate test for this comprehensive model of ticket splitting. On the typical American long ballot, voters have multiple opportunities to split their votes between the parties. We focus our attention upon the five statewide partisan races in the 1990 Ohio election.

Our first task is to estimate the extent of ticket splitting in the election. The conventional way to measure it is to determine the percentage of voters who split their tickets in each pair of races. The range of this measure in the 1990 Ohio elections is from a low of 23% to a high of 38% (Table 1). At the high end of this range, ticket splitting was higher than for president–House pairs in the Michigan surveys in recent years (Fiorina 1990), so that this election already evinces higher rates of splitting than the usual contests of reference.

The level of ticket splitting increases substantially when one examines voting patterns across all five contests. A total of 54% of the voters in our sample cast at least one vote for each party—a number that is virtually twice the often-reported magnitudes when

TABLE 1

Ticket Splitters as Percentage of Voters in Each Pair of Contests

	GOVERNOR	SECRETARY OF STATE	ATTORNEY GENERAL	AUDITOR	TREASURER
Governor	—	23 (487)	23 (422)	34 (440)	38 (392)
Secretary of State	—	—	25 (409)	32 (430)	30 (378)
Attorney General	—	—	—	36 (390)	33 (347)
Auditor	—	—	—	—	25 (363)

Note: Entries are percentages voting for candidates of different parties among those who voted in both contests of the pair. Ns are in parentheses.

only pairs of races are considered. That this level is attained in a strong party state (Fenton 1966, chap. 5; Mayhew 1986; Baum and Patterson 1992) in which both parties provide party slate cards that can be carried into the voting booth is perhaps surprising, even though the Ohio ballot does not permit a straight-party vote by a single action. The 1990 Ohio results show how widespread ticket splitting has become in American elections, and they document the high incidence of such behavior outside of presidential elections.

The analysis that follows concentrates on the patterns and sources of the split party vote across all five contests. The variable in focus is a simple one—whether or not the vote was divided between the parties. This is a dichotomous variable, yielding 53.5% ticket splitters and 46.5% straight ticket voters within the entire sample of voters.[6]

Our objective is to explain why some voters cast a straight party vote whereas others were motivated to divide their vote among candidates from different parties. One approach in answering this question is to consider that voters viewed each contest independently and, after weighing all important considerations, decided to vote for a candidate that turned out to be of the same or the opposite party from the other candidates preferred. In this explanation, each contest is treated as an independent event, requiring a more or less unique accounting. Furthermore, it does not presume a normal behavior of partisan voting from which deviations are to be explained.[7]

While the urge to examine voting behavior contest by contest is strong, particularly given the normal orientation of voting behavior studies and the understandable desire to try to maximize the amount of variance one can explain, our interest is in an alternative "whole ballot" approach that can account for the patterning of votes across the ballot. Only with this focus can a comprehensive theory of ticket splitting be developed.

We shall pursue two lines of analysis to develop this theory. First, we focus intensively on the two most promising variables in explaining the voting patterns, namely, partisanship and candidate visibility. The former is a key voter characteristic, while the latter contains the most salient feature of each contest. Thus, we bring together in the same analysis the two perspectives on ticket splitting that have characterized past research. Second, after we have carefully explored the effects of partisanship and visibility, we turn to a multivariate analysis of the split vote incorporating these and a number of other theoretically promising variables. By estimating a more fully specified model of ticket splitting than earlier studies, we hope to be able to determine the effects of party and candidates, as well as to sort out the relevance of other individual characteristics, especially those thought to characterize the "new" ticket splitters.

THE ROLE OF PARTISANSHIP IN VOTER TICKET SPLITTING

An examination of patterned voting in the contemporary American electorate must begin with the partisan loyalties of individual voters. The most visible—and often the only—link among candidates for office is their political party. In Ohio, this link is clearly identified on the ballot, so that even voters coming into the voting booth with absolutely no prior information about candidates receive this valuable voting cue. For voters who possess party identifications, the party of the candidates should be an especially potent cue; the stronger the voter's party identification, the more we should expect it to influence the voting choice.

The results of Table 2 support the hypothesis that party identification is related to a partisan pattern of voting in a powerful fashion. Strong Democrats and strong Republicans least frequently split their votes in the 1990 Ohio contests. A majority of them supported only the candidates of their party. Those who did divide their votes between the parties did so only minimally. Nonetheless, a substantial minority of strong partisans defected at least once, which suggests that strong partisanship alone is not sufficient to produce a straight party vote.

The threshold of party regularity is crossed once we move to weaker party-identifiers. Majorities of

TABLE 2

Party Identification and Split Ticket Voting, Ohio 1990 (%)

PARTY IDENTIFICATION	ALL D	SPLIT TICKET D > R	SPLIT TICKET D = R	SPLIT TICKET R > D	SPLIT TICKET TOTAL	ALL REP.	N
Strong Democrats	72	24	3	3	30	0	(81)
Weak Democrats	34	41	8	17	66	0	(92)
Ind. Democrats	31	26	14	21	61	7	(42)
Independents	9	49	15	25	89	2	(53)
Ind. Republicans	2	17	7	46	70	28	(54)
Weak Republicans	2	12	1	50	63	34	(90)
Strong Republicans	0	4	4	32	40	60	(92)

Note: N = 504. Pearson's r = .75/Kendall's Tau B = .63. Entries are the percentages for each category of partisanship who vote for all Democrats, all Republicans, or split their tickets. Ticket splitters are divided into those who voted for more Democrats than Republicans, equal numbers of each, and more Republicans than Democrats. Entries were made for voters who cast ballots in two to five of the five contests. Single-contest voters were eliminated because it was impossible for them to split their tickets.

less-than-strong partisans in every category split their votes between the parties, and almost all pure independents were split-ticket voters. Weak partisans and independent-leaners, in fact, behave more like pure independents than strong partisans. These two kinds of partisans, moreover, exhibit such similar patterns of voting behavior that in the subsequent multivariate analysis they will be combined.[8]

One other feature of Table 2 bears mention. In this particular election, strong Republicans were more likely to split their tickets than were strong Democrats. As we shall see, this is entirely attributable to the disproportionate Democratic advantage in incumbency, which that party has enjoyed for the last several decades throughout the country. What we observe in the 1990 Ohio elections, therefore, is an illustration of a general pattern of subpresidential voting: Republicans are more likely to be ticket splitters.

The reasons why partisanship plays such an important role in inhibiting ticket splitting are familiar. Viewed from the perspective of *The American Voter*, partisanship is an indicator of identification with the party as a group (Campbell, Converse, Miller, and Stokes 1960). Support for candidates from that party is an expression of group loyalty and should become more complete as one's party identification strengthens. The same result emerges when partisanship is viewed, from the rational choice perspective, as a valuable voting cue in an environment of imperfect information (Downs 1957; Fiorina 1981). Without information to the contrary, partisans assume that the candidates of their party share their own outlooks and preferences. In the relatively low information context of subpresidential races, then, partisanship should be a particularly valuable guide to voting. While these two perspectives on the role of partisanship in voting behavior rely upon different cognitive processes for explanation, they predict the same result for ticket splitting. Consequently, it is not necessary to choose between these explanations; nor is it possible with the data at hand.

PARTY, CANDIDATE VISIBILITY, AND TICKET SPLITTING

Candidate characteristics, of course, can overwhelm partisanship. The first sign of this is the integral role of incumbency in ticket splitting in the 1990 Ohio races. Almost three-quarters (72%) of the split-ticket voters in our sample divided their votes between the two parties by deviating from a preponderantly Republican voting pattern to support Democratic officeholders seeking reelection. Nearly two-thirds (64%) of these incumbent-oriented ticket splitters voted Republican for governor, then voted for at least one of the Democratic incumbents for other executive offices. This incumbent-seeking behavior, however, does not tell us why incumbency is so potent a force. For a preliminary answer to this question, we move momentarily away from a focus on the whole ticket to examine individual contests and how candidates in them may be advantaged by the visibility they enjoy in the voters' minds.

Our treatment of the candidate factor focuses on candidate differentials in visibility as perceived by voters. Visibility differentials can have many sources, which are worthy of exploration; but in the end, all that matters is *how* (not why) the voters see the candidates. The measure of perceived candidate visibility we employ is based on a series of questions from the postelection survey asking the respondent to evaluate the candidates on a variety of policy issues, personal traits, and group attachments. (See the Appendix for the text of these and other questions we use.) Each evaluation was scored as an indication of visibility. A candidate's overall visibility to the respondent was the sum of these particular evaluations, and the candidate with the larger total in each race was credited with the visibility advantage for that respondent. Visibility advantage (or disadvantage) is the simple difference between the number of respondents who knew a candidate better and the number who found the opponent more visible.[9]

TABLE 3

The Visibility of Candidates in the Ohio 1990 Election (%)

	VISIBILITY			
OFFICE	REPUBLICAN MORE	EQUAL	DEMOCRAT MORE	N
Governor	21	67	12	428
Secretary of State	19	74	7	421
Attorney General	16	71	13	396
Auditor	4	64	32	412
Treasurer	3	70	27	377

Note: Includes only respondents who voted in each contest. The figures for incumbents are underlined.

The relative visibility advantages in each contest, presented in Table 3, describe a common pattern. The incumbents seeking reelection to the auditor's and treasurer's offices enjoyed substantial visibility advantages over their challengers. That the incumbent secretary of state did not enjoy a similar edge is explained by the fact that the Republican challenger, Robert Taft, was a scion of Ohio's most famous political family. Taft *began* the campaign with high visibility and ended up with a substantial net edge over his incumbent opponent. Thus, our Ohio data contain one case in which a challenger was able to overcome the natural incumbent advantage; and this case will play a crucial role in specifying the nature of the usual incumbency advantage.

Our hypothesis is that these visibility advantages gained extra votes for the candidates who enjoyed them. Testing this hypothesis is no simple matter, however, because the vote gains must be assessed against some kind of baseline. One way to demonstrate the effects of visibility is to examine the relationship between visibility and vote, *controlling for party identification* as the baseline. These relationships are presented in Table 4. If visibility has no independent effect over and above partisanship, then these correlations should be uniformly insignificant. Where visibility has an impact beyond partisanship, by contrast, the correlations should attain significance and should, if visibility has the advantage we expect, be positive in direction.

The results in Table 4 show that candidates gained votes from a visibility advantage that transcended the effects of party identification. Almost all of the correlations are positive, and 14 of the 25 attain statistical significance. The only negative relationships are registered for independents, but they fall far short of significance. The visibility advantage was most pronounced in the three races with the greatest visibility discrepancy between the two candidates. Some of the largest correlations between visibility and vote appear among Democrats in the secretary of state race and among Republicans in the auditor and treasurer contests, which demonstrates the power of visibility in overwhelming partisan attachments. That in one of these contests the challenger, rather than the incumbent, was benefited by visibility, however, suggests (with Jacobson 1989) that incumbency does not guarantee a visibility advantage.

Exactly how visibility influences the vote is so far left unspecified. Candidates' characteristics always have been important in American elections; and with the weakening of partisanship in recent years, their importance seems to have grown. In many subpresidential elections—in contrast to the more familiar presidential contests—the amount of information citizens possess about candidates is so low that virtually any information the voters have may be critical in their vote decision. Since this information is typically favorable, sheer visibility often produces an electoral advantage. Consequently, the difference in what voters know about candidates is an important variable in accounting for vote decisions, even without further refinement as to the exact content and direction of that knowledge (Jacobson 1987).

TABLE 4

Effects of Candidate Visibility Differences on Vote by Party Identification

OFFICE	STRONG DEM.	WEAK/IND. DEM.	PURE IND.	WEAK/IND. REP.	STRONG REP.
Governor	.11	.25*	.13*	.09	.04
	(72)	(111)	(38)	(106)	(83)
Secretary of State	.41*	.18*	.43*	.18*	.13
	(71)	(110)	(39)	(105)	(82)
Attorney General	.23*	.30*	−.07	.08	.08
	(67)	(102)	(34)	(102)	(78)
Auditor	.07	.19*	−.06	.38*	.24*
	(70)	(111)	(36)	(101)	(79)
Treasurer	.08	.18*	−.08	.33*	.43*
	(68)	(100)	(36)	(91)	(70)

Note: Entries are Pearson correlations between candidate visibility differentials and vote within each partisan category for those who voted in four or five contests. To keep the number of cases in each cell relatively high, weak and leaning partisans, who behave similarly, are combined. Ns are in parentheses.
*$p \leq .05$ (two-tailed test).

This, surely, is the source of the well-known incumbency effect. Officeholders have been in the spotlight of usually favorable public attention in previous years and are able to marshal crucial vote-getting resources (e.g., money, media coverage, an already-established organization) during an election campaign. Few challengers can match—much less overcome—the visibility incumbents naturally enjoy. When challengers can erase the incumbent visibility advantage, however, they are likely to win the election (Jacobson 1989). Looking across the ballot, then, when there is variation in candidate visibility by party (as is typically the case), one would expect a divided party vote.

Any comprehensive analysis of split-ticket voting must take into account the nature of the candidates for office and how their public recognition can be an electoral asset or debit. On a case-by-case basis, we have shown that the sheer visibility of a candidate can overcome the normal partisan tendencies of voters. What remains to be done is to integrate this factor systematically into a comprehensive explanation of ticket splitting. To accomplish this task, we turn to multivariate analysis and to a more complicated measure of visibility.

A MULTIVARIATE ANALYSIS OF THE SOURCES OF TICKET SPLITTING

The analysis so far has identified partisanship and candidate visibility as key determinants of ticket splitting. As the representatives of the alternative theoretical foci on voter and candidate characteristics, they are the two leading candidates in an account of the division of the vote between parties across a long ballot. However, they are not the only promising explanations of ticket-splitting behavior. Previous studies have identified other reasons why voters may divide their votes between the parties, and we need to expand our analysis to include them. In order to assess these alternative explanations in conjunction with the partisanship and visibility accountings we have relied upon so far, a full multivariate analysis is required. The dependent variable in this analysis is the dichotomous measure of whether or not the respondent split his or her ticket.

The first variable added to partisanship and visibility in the multivariate analysis is age (measured in years). Some previous research has suggested that younger voters are more inclined than older voters to split their tickets. This behavior may lend itself to a life-cycle explanation. But the contention that the "new" ticket splitters are to be disproportionately found among the young suggests that there is something particular about this *generation* of young voters (DeVries and Tarrance 1972). One reason why the young may be more likely to cast divided party votes, of course, is that their partisan loyalties are weaker, for either life-cycle or generational reasons. By assessing the effects of age in a multivariate model that contains partisan intensity as a variable, we can examine this interpretation of the relationship of age to ticket splitting.

A second explanation for ticket splitting that merits representation in our multivariate model stems from the notion that cognitively sophisticated voters are less dependent on decision-making shortcuts. Their vote is more likely to be based on candidate-by-candidate evaluations, instead of relying on such heuristics as party or even the coattails of the top-of-the-ticket candidate (Chubb 1988; Shively 1979). Cognitive sophistication is captured in a crude but meaningful basis by education, measured in years of schooling. Education has been variously related to ticket splitting in previous research: better-educated voters were more likely to vote straight party in the Campbell and Miller (1957) study but more likely to *split* their votes in the DeVries and Tarrance (1972) analysis. By including education in our multivariate model, we can help to reconcile these divergent results.

Political sophistication is not simply a function of general cognitive ability (Converse 1964, 1980). Some people at each level of education pay inordinate attention to politics and, as a result, are able to bring considerable information to bear on their political decisions. This political specialization may be reflected in self-reports of political interest at the end of the campaign. Maddox and Nimmo (1981) found that ticket splitting was higher among those with the greatest political interest. There are good reasons to pursue this line of analysis here. To test the hypothesis that political interest induces a split ballot, we include the variable INTEREST, which varies from *not much interested* (scored 1) to *very much interested* (scored 3) in the campaign.

Third, ticket splitting may be a function of how people receive their information about politics. Television has become a key source of political information for almost all Americans. With its reliance on visual images, this medium is seen as fostering a personalization of politics, an emphasis on candidates over issues or party (Popkin 1991; Rubin 1981). Consequently, voters who derive their political information primarily from television may be expected to be more candidate-oriented, which leaves them more inclined to divide their votes between the parties across the ballot (Maddox and Nimmo 1981). By this logic, it is the increasing dominance of television as an information source in recent decades that accounts for the rise in ticket splitting.

This television-centered explanation for ticket splitting, too, warrants inclusion in our multivariate model. Because television use is universal, we represent it by determining whether respondents report a greater reliance on television as a source of their political information than either newspapers or personal discussants. The variable is TVDOM, scored 1 if television is favored and 0 if it is not. Those who rely upon television most, this hypothesis claims, should be more likely to split their tickets.

Fourth, some scholars have seen split-ticket voting as strategic behavior, in which voters who lack confidence in, or support for, either party are rationally motivated to divide their votes so that they can prevent one party from gaining full control of government (Alesina and Rosenthal 1989; Fiorina 1990). A test of this explanation for ticket splitting can be fashioned by employing attitudes toward divided government. We employ an agree–disagree question about its desirability, SPLITCON—scored from *strongly disagree* (1), to *strongly agree* (4), with the ideal of one-party control—that can be thought of as a general orientation because it was asked at the beginning of the 1990 campaign. If votes are motivated by a conscious desire to divide executive positions between the two parties, then this variable should be significantly related to ticket splitting.

Finally, the party identification and visibility variables are each represented in our model. Party is measured in terms of strength (PTYSTR) by folding the seven-point party identification scale at its independent midpoint and combining the virtually indistinguishable categories of weak partisan and independent-leaner. (The result is 1 = independent, 2 = leaning partisan/weak partisan, 3 = strong partisan.) Based on the results of previous research, partisan strength is expected to be significantly related to ticket splitting (as Table 2 demonstrates clearly in the bivariate case).

Table 2 also exhibits a tendency for Republicans to split their tickets more than Democrats in the 1990 Ohio elections. We hypothesize that this tendency can be accounted for by the generally higher visibility of Democratic candidates in the election and perhaps some of the other factors we have identified. To test this hypothesis fully requires the inclusion of a Republican partisan direction variable (REP, scored 1 if weak or strong Republican, 0 if independent, independent-leaner, or Democrat) in our model, so that we can determine whether its relationship to ticket splitting vanishes once the effects of other variables are taken into account, as we expect.

The measurement of perceived candidate visibility across the subpresidential ballot is more complex. Earlier, the effects of visibility in individual contests were inferred from the relationship between a visibility difference between candidates and the vote with controls for party identification. The conceptualization of visibility as it operates across the ballot necessarily takes this logic one step further. It depends upon the notion that the pressures to split a ticket are increased when candidates from different parties enjoy the relative visibility advantages across the ticket. The variable that expresses these expectations, called VISSPLIT, is scored 1 when candidates from different parties are more visible to the voter and 0 when all of the more visible candidates come from the same party.

Analysis of Voters in at Least Two Races

Table 5 presents the results of a multivariate analysis of the split ticket vote dichotomy on the variables just identified (see the Appendix for the questions underlying these measures) for those who have voted in more than one race.[10] These results confirm the dominance of partisan strength (PTYSTR) in accounting for split-ticket voting. No other variable is as strongly associated with ticket splitting behavior. It is hardly surprising that departures from a single-party vote decrease as the voter becomes more strongly committed to the party and that this commitment is the primary predictor of party steadfastness in voting. This relationship also makes the contemporary increase in ticket splitting more comprehensible. As partisan intensity has declined in recent years, it surely has brought with it a parallel decline in party-ticket voting.

TABLE 5

Probit Analysis of Ticket Splitting

INDEPENDENT VARIABLE	b	S.E.	CHANGE IN PROB. OF SPLITTING
PTYSTR	−.79***	.112	.09
REPUBLICAN	−.02	.125	—
VISSPLIT	.62***	.160	.10
SPLITCON	.05	.128	—
TVDOM	−.22	.133	—
INTEREST	−.13	.099	—
EDUCATION	.07**	.028	.01
AGE	.01**	.004	.01
Constant	1.22	.670	—

Note: N = 463. McKelvey Pseudo R^2 = .29; Dhrymes Pseudo R^2 = .15; likelihood ratio = 83.18 (approx. chi-square with 9 d.f.); percentage of cases predicted accurately = 70%. The table excludes those who voted in only one contest. The dependent variable in this analysis is ticket splitting, scored 1 if vote was for candidates from two different parties and 0 if for candidates from only one party. Probabilities of splitting are the changes in the probability of split-ticket voting associated with a one-unit increase in that variable with every other variable held at its mean. Probabilities are not shown for variables whose impact on split-ticket voting fails to achieve statistical significance. In calculating the percentage of cases predicted accurately, the prediction is straight ticket if estimated p is less than .50 and split-ticket if estimated p is at least .50.

**$p \le .01$ (one-tailed test).

***$p \le .001$ (one-tailed test).

The greater tendency for Republicans to split their tickets that appeared earlier in the simple relationship between partisanship and ticket splitting vanishes in a multivariate context. Republicans are more likely to vote for candidates from both parties; but the results suggest that the reason lies in factors other than their partisanship—chiefly, we surmise, in the advantages Democratic incumbents enjoy in visibility.

Candidate visibility joins partisan strength in the multivariate analysis as a potent predictor of ticket splitting. Voters who saw at least one candidate from each party as more visible across the five races were significantly more likely to split their tickets than those for whom the visibility advantage always remained with the same party. Incumbency no doubt explains a substantial part of this effect, since incumbents are best able to overcome the natural advantage held by candidates of a voter's own party or to monopolize the attention of independents. But—as the presence of a visible Republican challenger in Robert Taft makes it possible to show—it is differential visibility rather than incumbency per se that is conducive to a divided party ballot.

Only two other variables exhibited significant relationships to ticket splitting, and the strength of these relationships fell far short of those for visibility and partisan strength. One of these variables is education, in which a one-year increase in attainment was associated with a .01 increase in the probability of ticket splitting. Unlike the results for the 1950s (e.g., Campbell and Miller 1957) but consonant with more recent findings (e.g., DeVries and Tarrance 1972), more educated respondents were more likely to split their tickets in the 1990 Ohio elections. Because the education of the electorate has been growing in recent decades, the overall importance of this relationship probably transcends its coefficient. The case for a "new" ticket splitter finds some support in the reversal of the old voting patterns by those with higher levels of education.

Split-ticket voting is also more common among older voters. This is an unanticipated result from the standpoint of both earlier research and theoretical expectations. Younger voters are normally expected to be less firmly anchored in their voting behavior. They tend to be less partisan than their elders, although there has been a surge of the youngest age group into partisanship in more recent years. They also are better educated. Both of these factors should predispose them to split their tickets more; yet even the bivariate correlation between age and ticket splitting is positive, not negative. Moreover, the magnitude of this positive relationship increases once other variables, including partisanship and education, are taken into account. The image of the "new" ticket splitters as disproportionately young does not bear up under closer scrutiny in our data.[11]

The other hypotheses tendered to explain ticket splitting fail to survive our analysis. The image of ticket splitters as television-dependent is undermined, at least in Ohio for 1990, by the fact that respondents for whom television dominates newspapers and personal discussions as a source of political information *less* frequently divide their votes between the parties (although this relationship falls short of significance).

Nor does the hypothesis that ticket splitters are rationally dividing governmental powers between the parties (Fiorina 1990) receive support in our data. The attitude that government works better under conditions of divided control bears no relationship to ticket splitting, even after a host of other factors predisposing the voter toward dividing the vote are taken into account. This result accords with findings from another contemporary study (Alvarez and Schousen 1991), which makes it all the more likely that the divided-government-motivated-voter hypothesis lacks explanatory power.

Another hypothesis that emerged from earlier research is that voters with higher levels of interest in politics were more likely to split their ballots. This hypothesis, too, receives no support in our analysis. As interest increases, frequency of ticket splitting declines—rather than increasing, as previous research had led us to expect it would. But the relationship is so modest that it falls far short of achieving significance.

Demonstrating the Robustness of the Results

Ticket splitting has proven to be a more complicated phenomenon than it appeared to be at first blush. Additional analysis is needed to make certain that these complications have not produced anomalous results. One complication is created by widespread *roll-off* (the tendency not to vote for some contests—see Burnham 1965) in the 1990 Ohio elections. The failure to cast a vote in all races on the ballot lowers the probability of ticket splitting. By restricting analysis to those voters who cast ballots in two or more contests, Table 5 has eliminated the complication imposed by those for whom the probability of a split ballot was zero. But 115 voters (almost 20% of all the voters in our sample) cast ballots in only two or three contests, thereby severely restricting their opportunities to divide their votes between parties. We shall have more confidence if our results hold up after these voters, too, are removed.

Another complication is created by the possibility that idiosyncratic forces may play upon the vote. Deviations to vote for the other party by the erstwhile party voter can occur for a variety of reasons, such as a personal friendship, a local "favorite son or daughter," a key issue, or a personal scandal. While the resulting votes are appropriately counted as divided between the parties, a peculiar distribution of these idiosyncrasies in an election might give a distorted picture of the sources of ticket splitting. It is probably safe to assume that these reasons are randomly distributed across contests and years, so that they do not confound our results. To guard against the possibility of this kind of bias, however, we have used the more stringent comparison of those who voted for at least two candidates from each party, as opposed to straight-ticket voters.

Table 6 presents the results obtained from these two different lines of analysis. What is most striking about these results is that the coefficients for the partisan strength and visibility variables change hardly at all from those in Table 5. These two variables appear to be powerful sources of ticket splitting regardless of the circumstances. The same cannot be said, however, for age and education. While virtually unaffected by increases in the opportunities for ticket splitting, these coefficients decline and become insignificant among the most extensive ticket splitters, namely, those who have voted for at least two candidates from each of the parties. Only partisan strength and differential candidate visibility are related to this extreme form of ticket splitting.

All in all, this analysis increases confidence that our results are not an artifact of a measure that is overly

TABLE 6

Probit Analysis of Ticket Splitting for Voters in Four or Five Races

INDEPENDENT VARIABLE	ONE SPLIT		TWO SPLITS	
	b	S.E.	*b*	S.E.
PTYSTR	−.86***	.127	−1.01***	.165
REPUBLICAN	.03	.158	−.03	.214
VISSPLIT	.62**	.173	.85***	.207
SPLITCON	−.01	.125	.06	.177
TVDOM	−.08	.157	.03	.188
INTEREST	−.13	.108	−.09	.150
EDUCATION	.08**	.032	.05	.038
AGE	.01*	.005	.00	.006
Constant	1.47*	.724	1.76*	.912
McKelvey Pseudo R^2	.31		.38	
Dhrymes Pseudo R^2	.16		.22	
Likelihood ratio (approx. chi-square with 8 d.f.)	83.18		71.44	
%Cases predicted accurately (%)	70.0		73.0	
N	385		249	

Note: Only voters in four or five races are included in this table. In column 1 the dependent ticket-splitting variable is scored 1 if the respondent voted for candidates from two different parties and 0 if for candidates from the same party. In column 2 it is scored 1 if the respondent voted for two candidates from each party and 0 if for candidates from the same party, with those who split their tickets only once eliminated. In calculating the percentage of cases predicted accurately, the prediction is straight-ticket if estimated p is less than .50 and split-ticket if estimated p is at least .50.

*$p \leq .05$ (one-tailed test).

**$p \leq .01$ (one-tailed test).

***$p \leq .001$ (one-tailed test).

sensitive to a sample that includes too many partial voters or to idiosyncratic behaviors. While other (albeit less theoretically justifiable) validity tests could be run, we are confident that they would yield similar support for the robustness of the results reported in Table 5.[12]

CONCLUSION

Divided control of government is as important a feature of the American states as it is of the national government. The results of the 1990 Ohio election could not have produced better evidence of this. When the dust had settled, the Republicans continued their control of the Ohio senate; the Democrats, their hold on the state House of Representatives. Furthermore, the Republican party broke the previous Democratic monopoly on executive positions by gaining the offices of governor and secretary of state, leaving the other three statewide executive positions in the hands of their Democratic rivals. Thus, the contests for executive office in Ohio in 1990 have provided us with an excellent vantage point from which to explore the electoral bases for divided government.

The traditional American long ballot, in a political structure and political culture that subvert party regularity, offers voters ample opportunity to support divided party control of government. A majority of the Ohio electorate in 1990 took advantage of this opportunity by casting ballots for candidates from each of the two major parties for the executive offices. The election's split outcomes are primarily (although not wholly, because of roll-off) attributable to this behavior.

Based on these results, it seems reasonable to suppose that split-ticket voting in American elections is far more common across the full ballot than previous studies focusing on pairs of contests or congressional–presidential divisions have implied. To adequately study ticket-splitting behavior, we believe that it is imperative to consider the full menu of contests placed before the voter in any single election, including both the rare high visibility contests and the commonplace races of low visibility. The heavy focus in previous research on splits in pairs of contests, especially the presidential–congressional set, risks both underestimation of the frequency of the behavior and misspecification of its sources.

Despite the fact that ticket splitting is often the result of independent decisions for the different contests on the ballot, clear sources of this behavior across the subpresidential ballot have been identified. The primary motivating factor for ticket splitting—contrary to the discovery of ticket splitters as a "new force" two decades ago (DeVries and Tarrance 1972)—is a long established and powerful force in American voting behavior, strength of party identification. Documented as the key determinant of the split-party vote as long ago as the 1950s (Campbell and Miller 1957), partisanship turns out to be the prime determinant of split-ticket voting in Ohio in 1990. This result would surely be replicated in elections at various levels of government across the nation.

A focus on multiple offices across the ballot enables us also to determine how different candidate pairings may affect ticket splitting. This analysis shows that *the same voters* react differently to candidates based solely on their relative visibility, without regard to the content of these candidate perceptions. Candidates who enjoy a visibility advantage are very successful in attracting votes beyond their own partisan camp and in solidifying the support of their fellow partisans. The major source of visibility is, of course, incumbency. Indeed, in the 1990 Ohio elections, the preponderance of split tickets were the result of the powerful pull of incumbency and the great advantages it conferred on two of the incumbent candidates. Nonetheless, because incumbency does not always ensure such an advantage (as our results have shown), the explanation should rest more generally upon candidate visibility.

Especially because these Ohio results parallel those from studies of congressional–presidential splits in important respects, we have little hesitation in generalizing beyond the confines of our specific data.

Incumbency is an important contributor to ticket splitting, but this is because of the visibility advantages it often confers and not some intrinsic quality of incumbency itself. The visibility edge induces opposite-party loyalists to defect from their party's candidates, pulls independents into the favored candidate's orbit, and reinforces the natural tendencies of fellow partisans. In Ohio in 1990, this phenomenon generally favored the election of Democrats; and it is likely that the same would hold true for other states and levels of government where the Democrats enjoy an incumbency advantage. If so, this "pull of incumbency" should be added to macro theories of electoral change.

Evidence in our results for the "new" ticket splitter described by DeVries and Tarrance (1972) is mixed. In accordance with the results of more recent studies, those who split their tickets in our sample were generally better educated than those who voted straight tickets, even after the effects of partisan intensity and visibility were taken into account. By contrast, the classic 1950s study of ticket splitting found it occurring disproportionately among the *least* educated voters (Campbell and Miller 1957). The effects of education, however, appear to apply only to single-contest deviations. Recurrent splitters (at least across the 1990 Ohio ballot) were not distinctive in their educational levels.

This "new" ticket splitter apparently does not include the youngest members of the electorate. The young in our study were more likely than older voters to vote *straight* tickets, rather than to divide their votes between candidates from different parties. We can hypothesize alternative reasons for this anomalous result. First, the relationship between ticket splitting and age is consonant with a situation in which realigning forces have produced a new generation of loyal partisans among the young without converting often-unfaithful older partisans from their established loyalties. Second, older voters may be more familiar with candidates who have been prominent on the political scene (but not just the Taft name, as we have shown); consequently, these voters are less dependent on the party cue. Whatever the explanation, the greater propensity of the young to divide their votes that appeared in previous studies was not replicated in the Ohio 1990 election.

Finally, the story of ticket splitting in Ohio during the 1990 elections can be told entirely without resort to some variables that have proven important in earlier studies. In particular, levels of interest in politics proved to have no significant relationship to voting for candidates from both parties. Neither did dominance of television as an information source or a motivation to produce divided government emerge as important. These results could be anomalies of our case, but we suspect that they are more likely to be typical results of the multivariate, multiple-vote approach that sheds more light on ticket splitting as patterned behavior.

A combination of motivation (partisanship) and opportunity (the visibility differentials of the candidates), with a pinch of education and age, then, accounts for a substantial portion of ticket splitting across the 1990 Ohio subpresidential ballot. To be sure, the nature of the ballot itself, by making it easier or harder to vote a straight ticket, can accentuate these effects. The Ohio office-block ballot, for example, makes it easier than in many states to split a vote between the parties. A cross-state study would have to take this structural feature into account. But in the end, any theory attempting to explain ticket splitting behavior would be wise to focus its attention on partisanship, candidate visibility, education, and age, paying special attention to the first two. To understand systematic patterns of ticket splitting, in short, requires a merging of the two separate research traditions that have characterized its study to this point.

APPENDIX: MEASUREMENT OF KEY VARIABLES

Votes. The measures of votes in the various contests are based on the questions, "Did you vote in the contest for ——— between ——— and ———?" and if *yes,* "Did you vote for ———, the Democrat, or ———, the Republican?"

Party Identification. The measures of party identification are based on the question, "Generally speaking, do you usually think of yourself as a Republican, a Democrat, an independent, or what?" and, if Republican or Democrat, "Would you call yourself a strong (Republican, Democrat) or not very strong (Republican, Democrat)?" or if independent or *no preference,* "Do you think of yourself as closer to the Republican party or to the Democratic party?"

Visibility. The measures of visibility are based on questions asking respondents to place each candidate on a five-point continuum for four issues that were important in the 1990 campaign (abortion, campaign fund-raising, taxes, and government regulation of business); on a five-point liberal–conservative continuum; on six traits (competent, honest, strong as a leader, caring about people, decent, hardworking); and on closeness to six groups (Catholics, business, Christian fundamentalists, unions, blacks, and feminists). The issue questions (too lengthy to list here) were similar in form to those used in the American National Election Study (ANES) with specific positions anchoring each pole. The trait questions were identical to the ANES questions. The group closeness questions were worded: "Now I want you to tell me if you see any of the candidates as *really close* to some groups in American society. The first candidate is ———. Do you see him as *really close* to ———?"

SPLITCON. This variable is based on responses to the statement "State government works best when one party controls all the statewide offices."

TVDOM. This variable is based on the combination of responses to three questions: "In an average week, how often do you read a daily newspaper?", "In an average week, how often do you watch the local television news?" and "Have you talked about the campaigns, the candidates, or the issues in the 1990 elections with anyone—such as a friend, neighbor, coworker, your spouse or other relative?" Where the respondent watched television regularly but did not regularly read newspapers or did not talk with anyone, television was coded as dominant.

Interest. This variable is based on the following question asked after the election: "Some people don't pay much attention to political campaigns. How about you? Would you say that you were very much interested, somewhat interested, or not much interested in following the campaigns this year?"

Notes

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1. This number includes Nebraska's nonpartisan unicameral legislature, which has long been controlled by Republicans. Nebraska currently has a Democratic governor.

2. Before the 1890s, each party prepared its own ballot in most of the states. Ticket splitting, consequently, required the taking of two party ballots in full public view and the combining of them in some fashion to register votes for each party. Given this requirement, it is understandable that ticket splitting was so rare prior to the turn of the century.

3. In the two open-seat contests, a Republican was elected governor, and a Democrat won as attorney general. Two of the three incumbents won reelection, with incumbent Democrats winning the races for treasurer and auditor and a Republican challenger being elected as secretary of state.

4. A total of 1,277 Ohio adults were interviewed in wave 1 of the panel, which was conducted in September and early-to-mid October. The response rate for this wave was 57%. (A wave 2 interview was conducted for 492 wave 1 respondents within two weeks of the election, but these data are not utilized in this analysis.) Of these wave 1 respondents, 1,033 completed wave 3 postelection interviews for a reinterview rate of 81%. Only 567 of the wave 3 respondents reported having voted in the election, however; and they are the major focus of analysis. This two-wave panel sample matches up well with the known characteristics of the Ohio adult population in gender, age, race, and county of residence (based on the 1990 census). Like most surveys, however, it overrepresents people with higher levels of education. This sample bias should be taken into account in generalizing from the results.

5. The studies were conducted by the Survey Research Center at the University of Michigan. Beginning in 1970, the Michigan surveys were conducted by the Center for Political Studies, which had devolved from the Survey Research Center. Since the mid-1970s, these studies also have been referred to as the American National Election Studies.

6. A total of 6% of the voters cast a ballot in only one race (usually for governor), while 6% and 12% voted in two and three contests, respectively. One-race voters could not split their tickets and the opportunities to split were low for two- and three-race voters as well, so it should not be surprising to find that the percentage of ticket splitters is considerably lower among these voters (36%) than among four- and five-race voters (59%). Roll-off could be construed as a form of ticket splitting, but we reserve this treatment for subsequent study.

7. In effect, our treatment assumes that a latent continuous variable (reflecting propensity to split the ticket) underlies the dichotomous split/straight categories. We did consider, however, a number of alternative functional forms for the dependent variable. Because our theoretical interest lay in allowing alternative foundations for ticket splitting to compete as explanations for a singular, across-the-ballot behavior, in the end we chose to focus more or less exclusively on a dichotomous dependent variable.

8. When the party identification measure is folded at its independent midpoint so that it ranges from strong partisans of either party to independents, the relationship (and the similarity between weak partisans and independent-leaners) becomes even clearer. The percentage of ticket splitters declines in step fashion (82%, 63%, 60%, 33%) from independent to leaners to weak partisans and finally to strong partisans; and the Pearson's $r = -.30$. All subsequent analyses were conducted by measuring partisan strength in alternative ways—with and without leaners and weak partisans combined. The results were virtually identical and yielded no differences of substantive importance. Because combining leaners and weak partisans produces a slightly greater impact for partisan strength and using dummy variables for each category (save one) produces the same result but is more complicated to explain, we report the estimates for the three-category variable.

9. Visibility is measured from the postelection surveys, because the preelection results (wave 1) yielded so many no-information ties between candidates that they had virtually no discriminatory power.

10. We exclude the 36 voters who cast ballots for only one contest so as not to confound our analysis with those for whom ticket splitting is a logical impossibility. Because the dependent variable is a dichotomy, these are probit estimates rather than ordinary least squares regression estimates. Probit has the further advantage of allowing us to translate the effects of each variable into probability terms. To accomplish this, we set all other predictors at their mean values and then compute the change in the predicted probability of a split ticket that would occur as a result of a one-unit increase in the explanatory variable of interest.

11. We have estimated a variety of different equations to try to eliminate this unexpected age effect, but it has proven to be robust throughout. In particular, we reestimated the equations in Tables 5 and 6 without the race including Bob Taft, to see if the attraction of the Taft name to older voters might have induced them to split their tickets in this one instance; but the results were unchanged.

12. It is important to keep in mind that the variations on the basic model reported here are only two of a number of alternative specifications, many of which may extend beyond simple restrictions on the sample or measurements of the dependent variable. Indeed, we hope that our work will stimulate future efforts at exploring alternative qualitative and quantitative representations of ticket splitting across the ballot.

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ADAPTIVE PARTIES IN SPATIAL ELECTIONS

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We develop a model of two-party spatial elections that departs from the standard model in three respects: parties' information about voters' preferences is limited to polls; parties can be either office-seeking or ideological; and parties are not perfect optimizers, that is, they are modelled as boundedly rational adaptive actors. We employ computer search algorithms to model the adaptive behavior of parties and show that three distinct search algorithms lead to similar results. Our findings suggest that convergence in spatial voting models is robust to variations in the intelligence of parties. We also find that an adaptive party in a complex issue space may not be able to defeat a well-positioned incumbent.

Since Anthony Downs's *Economic Theory of Democracy* (1957), a spatial theory of elections has occupied a prominent theoretical status within political science. Modelers use the intuitive notion of ideological distance to develop explanations for observable electoral trends. The most famous of these trends is the Downsian idea that in a two-party system, given certain assumptions, parties converge toward a median position on the continuum of possible voter positions. Although simple spatial models have produced this result, extensions to the models have questioned the robustness of this prediction. Following the voting paradox and the results of Plott (1967) and McKelvey (1976), some scholars have speculated that chaotic results are possible and, in some cases, likely. In two or more dimensions, the top cycle set consists of the entire space of issue positions.[1] Bates summarizes the more general result: "The principle lesson is that, in general, one cannot expect an equilibrium to exist; and, because any outcome can be defeated, political decisions represent arbitrary outcomes" (1990, 45). Whereas some scholars concede the predicted instability in multidimensional voting models (Riker 1982), others see the Downsian model's stability and convergence as more empirically accurate and (perhaps) more normatively desirable.

Some theorists, inclined to believe that electoral chaos is extremely unlikely, have incorporated various complexities to explain stable, often centrist, outcomes. Coughlin (1990a) divides these models into four general categories: (1) models that allow for mixed strategies by parties, (2) models that track dynamic trajectories of party locations, (3) models that search for uncovered or undominated sets, and (4) models that include candidate uncertainty over voters' behavior (probabilistic voting models). He writes, "It is hard to resist the alternative inference that the primary contribution of recent work on the majority rule relation is as a grand 'reductio ad absurdum' that tells us to go back to the basic model that has been used to see how it should be modified in order for theory and empirical observations to match up" (p. 164).

Two-party electoral outcomes appear more stable than the chaotic results predict, so the task of seeking alternative assumptions to get more realistic outcomes has a sound scientific basis. Nevertheless, both original spatial models and many contemporary revisions rely on unrealistic assumptions to produce equilibria. They often assume fully informed and optimizing voters and parties (Davis, Hinich, and Ordeshook 1970). Spatial modeling seems wedded to the defining assumptions of rational choice, namely, that people and organizations are self-interested, have complete information, and can locate optimal strategies regardless of complexity. Chaos results, for example, rely on unrealistic assumptions about the abilities of parties to locate winning platforms. Probabilistic voting models assume that parties position themselves optimally given complete knowledge of the probabilities of voters' actions (Coughlin 1990b). We find implausible even the restricted assumptions that parties have sufficient information and analytical abilities to locate optimally. Perhaps different assumptions about the abilities of parties or voters lead to stable outcomes.

We incorporate boundedly rational parties in a model of two-party competition. Instead of modeling parties as full-information global maximizers, we model them as incompletely informed and adaptive. They move incrementally toward better regions of the space through the use of search algorithms. Our model involves a dynamic interaction between parties who make decisions in an evolving environment. Much like Kramer (1977), we are interested in the trajectory of party platforms over a sequence of elections. Using our methodology, we are able to explore a variety of questions: Do imposed informational and computational constraints lead to arbitrary outcomes, with winning party platforms scattered throughout the space? Do boundedly rational parties converge toward centrist platforms? Do parties always defeat incumbents, as many models suggest? Does altering the preferences of parties from vote maximizing to winning with ideals affect the behavior of parties? Our use of adaptive agents allows us to address these questions without wholly abandoning formal modeling.

We shall discuss the advantages of using artificial adaptive agents in the social sciences and present our basic model and a description of the two types of party preferences that we consider. We then present the three search procedures used by our parties and describe our results. We conclude with a discussion of the role of information in elections and possible directions for future research.

THE USE OF ARTIFICIAL ADAPTIVE AGENTS

Holland and Miller advocate a class of models using artificial adaptive agents (AAA), a technique in which "the unfolding behavior of the models can be observed step-by-step" (1991, 366). Based on computer languages, these models are flexible enough to capture a variety of situations yet they maintain precision and logical consistency.[2] Computational models allow the exploration of systems of well-defined agents in a perfectly controlled environment that can be easily and rapidly replicated under many conditions. Moreover, any conceivable analysis is feasible since the state of the system is fully recoverable. This allows the researcher to generate, develop, and test theoretical hypotheses quickly. Although optimization is the key benchmark in much social science theory, learning, adaptation, and bounded rationality are recognized as important processes. Through the use of AAA models, questions about the relationship between optimization and adaptation can be explicitly explored. We adopt the AAA technique because of these advantages and because of our belief that political parties are better characterized as partially informed, finite-memory information processors who rely on rules of thumb than as completely informed rational agents.

Underlying our methodology is the notion that there exist important classes of behavior that can be captured in models too complex for traditional mathematical analysis. Absence of equilibria in a model (or equilibria that human agents could locate only by chance) does not necessarily imply a lack of predictability. AAA allow us to search previously inaccessible models for patterns of generic behavior. Our choice of technique has costs. Restriction to any one type of algorithm can yield ad hoc results, and predictions are often less precise than those of rational actor models. To control for these possibilities, we compare three distinct types of AAA, and arrive at strikingly similar results for all three. The use of multiple algorithms to discover equivalence classes of adaptive behavior distinguishes our model from others and strengthens our results.

There are many precedents for the use of AAA and similar techniques in the social and behavioral sciences. For example, economists have applied AAA to evolve efficient bilateral trading rules (Marimon, McGrattan, and Sargent 1990) and to explore learning in varying economic environments (Arifovic 1989; Miller 1986). In political science, Cohen (1984) has used computer models to explore competing theories of organizational decision making, and Whicker and Strickland (1990) have used computer simulations to test the importance of opinion distribution and decision rules on the constitutional amendment process. Axelrod (1986) has simulated the evolution of norms and has also used a genetic algorithm to develop new strategies that were superior to Tit for Tat in his 1984 computer tournaments (Axelrod 1987). One of the present authors, Miller (1987), has analyzed the coevolution of strategies and the emergence of cooperation in noisy repeated prisoner's dilemma games using adaptive algorithms.

Thus, we agree with Coleman (1989) that simulation models with humans, computers, or both can be useful in the construction of social theory. We shall use computer models to trace the emergent behavior of boundedly rational parties in the context of spatial voting models. Since our parties are limited in both the information they possess and in how they process the information, there is no guarantee that in finite time they will find optimal locations in the policy space. We can, however, compare the general movements of the parties over time to the predictions of mathematical models of party competition. The model we put forth does not stretch the boundaries of our technique. Our present purposes—to address a central problem in positive political theory and show the strength of our approach—are best served by a simple model.

THE BASIC MODEL

Our model incorporates many of the assumptions of spatial voting models, including voters who are perfectly informed about candidate platforms. We follow more recent models and relax the assumption of identical voter preference intensities. We call a voter's preference intensity a *strength*, interpreted as the importance that the voter attaches to an issue. Strengths vary, so voters have noncircular indifference curves. Our model departs from standard spatial models in three respects. First, we consider both purely office-seeking parties and ideological parties. Second, our parties are not perfectly informed, and do not know individual voter utility functions. Rather, they obtain information through "test" elections (like opinion polls) that measure how well their current platform would do relative to their opponent's platform. During a campaign, parties test their platforms on voters, receive feedback in the form of vote totals, and alter their platforms to improve vote totals. Finally (as discussed), our parties do not optimize given their limited information. Instead, using the vote totals from test elections, they adaptively alter their platforms, trying to defeat their opponent.

Formally, there are two parties competing for V votes in an n-dimensional issue space. Each voter's preferences are represented by two vectors of n integers, which give the voter's ideal positions and strengths on the n issues. We assume that there are k possible positions on each issue $\{0, 1, \ldots, k-1\}$ and s possible strengths $\{0, 1, \ldots, s-1\}$. The utility to a voter from a party's platform, $y \in \{0, 1, \ldots, k-1\}^n$, is given by the negative of the squared weighted Euclidean distance, where the weight on the ith issue is the strength associated with that issue by the voter. If s_{ji} denotes the jth voter's strength on the ith issue and x_{ji} is the jth voter's ideal point, then a voter's utility is given by

$$u_j(y) = -\sum_{i=1}^{n} s_{ji} \cdot (x_{ji} - y_i)^2$$

In this model we assume that both strengths and ideal points are independently and uniformly distributed. The election results we present consider 251 voters, 15 issues, 7 positions per issue, and 3 possible strengths. Therefore, on average, each voter has five issues of major importance ($s_{ji} = 2$), five of minor importance ($s_{ji} = 1$), and five of no importance ($s_{ij} = 0$). The assumption that voter ideal points are uniformly distributed does not necessarily imply regularity. A relatively small number of voters are generated in a large space, so a spray of points is a more appropriate way to think of the distribution.[3] Each voter casts a ballot for the party giving the higher personal utility. To evaluate the trajectory of democratic outcomes, we introduce *centrality*, a measure of the goodness of each outcome. Without such a measure, we cannot compare our model analytically to any other model. We calculate centrality by computing both the sum of the utilities of the individual voters if the winning party were located at the median on all issues and the sum of utilities resulting from the winning party in the election, then divide the former by the latter.

$$c(y) = \left[\sum_{j=1}^{V} u_j(\text{median})\right] \Big/ \left[\sum_{j=1}^{V} u_j(y)\right]$$

It follows that $c(\text{median}) = 1$. This normalization has the interpretation that the higher the centrality, the closer the winning candidate is to the weighted center of voter preferences and therefore the more responsive the democratic outcome. We attach no normative significance to the median itself as an outcome. We merely exploit the fact that it is generally of high aggregate utility. There may exist platforms with centralities greater than 1. Ideally, we would find the platform of minimal average distance and use its utility as the numerator; but the costs in computer time outweigh any advantages. Regardless of the numerator, we have a measure of aggregate utility, or the average weighted distance to a voter.

Parties are initially represented by a randomly selected "ideal" platform. Since parties are aggregates of individuals, we assume they have uniform strengths across all issues. We consider two types of parties: *ambitious* and *ideological*. Ambitious parties care only about winning elections, and their ideal platform serves only as a starting point for the initial campaign. Ideological parties also want to win the elections, but they want to win with a platform that is spatially close to their ideal platform. Formally, if $v(\boldsymbol{y}{:}\boldsymbol{x})$ is the number of votes the challenger party receives if it takes platform $\boldsymbol{y}$ and the incumbent is at $\boldsymbol{x}$, and $\boldsymbol{Y}$ is the challenger party's ideal point, then the objective functions for the ambitious and ideological parties can be written as:

$$\text{Obj}_{\text{am}}(y) = v(\boldsymbol{y}{:}\boldsymbol{x})$$

$$\text{Obj}_{\text{id}}(y) = \begin{cases} v(\boldsymbol{y}{:}\boldsymbol{x}) & \text{if } v(\boldsymbol{y}, \boldsymbol{x}) \le V/2 \\ V/2 + n \cdot k^2 - \sum_{i=1}^{n} (y_i - Y_i)^2 & \text{if } v(\boldsymbol{y}{:}\boldsymbol{x}) > V/2. \end{cases}$$

Recall that V is the total number of voters, n the number of issues, and k the number of positions per issue. This implies that

$$n \cdot k^2 \ge \sum_{i=1}^{n} (y_i - Y_i)^2$$

for any y and $\boldsymbol{Y}$. Thus, ambitious parties attempt to maximize votes in the hopes that a larger margin of victory makes them more difficult to defeat in subsequent elections, while ideological parties have lexicographic preferences. Their primary goal is to win the election: $v(\boldsymbol{y}{:}\boldsymbol{x}) > V/2$. Once this is accomplished, they attempt to get as close to their ideal platform as possible.[4]

During each election, the incumbent party's platform is fixed, and the challenger party attempts to find a platform in the issue space that defeats the incumbent. In the first election the incumbent party (arbitrarily chosen) remains at its ideal platform. Thereafter, the incumbent remains at the platform where it won the previous election. The challenger party attempts to defeat the incumbent by choosing a new platform. The challenger party, during a finite campaign, tests new platforms on voters who are assumed to have perfect information about both platforms. These tests are accurate polls of political popularity.

Both ambitious and ideological parties are constrained in how they search the issue space for good platforms. First, the campaigns are of finite length, so parties are limited in the number of polls they can take. For example, a party may only be able to take 40 polls before the election. Second, during any platform adaptation, our parties are limited by the number of issues they can change and the degree of change on any such issue.[5] Even for ambitious parties, the ideal platform functions as an ideological tether in early elections.

Positioning constraints and finite campaign length imply that our parties fail to fulfill their goals optimally. Ideological challengers should typically win elections because ideological incumbents attempt to stay near their ideal platforms, which generally lie in regions of average centrality. However, because of their limited ability and information, ideological challengers accept platforms further from their ideal platforms than necessary. With ambitious parties, after a few elections the incumbents' platforms are located in regions of high centrality, making them difficult to defeat. Therefore, ambitious challengers often lose.

HOW PARTIES FIND PLATFORMS

Once we relax the assumptions that parties have complete information and the ability to locate optimal platforms, we can model our parties in a variety of ways. There are many ways to not be perfectly rational. We chose three types of parties, each having a different platform search procedure: *random adaptive parties* (RAPs), *climbing adaptive parties* (CAPs), and *genetic adaptive parties* (GAPs). The search procedures were constructed to be crude approximations of actual procedures. More important, they provide reasonable bounds on the ability of parties to locate platforms. The procedures themselves are mechanisms for the party to choose the platform it presents to the voters against the incumbent. All three procedures will be discussed primarily within the context of ambitious parties. The extension to ideological parties is straightforward.

The RAPs are the least adaptive of our parties. Letting L represent the length of the campaign, RAPs randomly generate L platforms in the neighborhood of their previous platform and choose the platform that receives the most votes against the incumbent. The RAPs approximate a smoke-filled-room selection process. The party gathers immutable potential candidates and selects the highest vote-getter to represent the party in the election. We do not dispute the contention that this underestimates the ability of parties or candidates to adjust to public opinion. The RAPs are intended as lower bounds on the ability of parties to position themselves.

In contrast to the RAPs, both the CAPs and GAPs selectively refine their platforms to improve vote totals. The CAPs begin with their current platform and experiment, slightly changing positions on a few issues. If the new platform fares better against the incumbent than did the previous one, the party switches to the new platform. These platform tests are called hill-climbing iterations. Here, the campaign length L equals the number of hill-climbing iterations (including those resulting in no improvement) that a party performs before the actual election. The CAPs enter the election with their final—and therefore best-to-date—platform. The CAPs represent parties that select a candidate and then adapt the candidate's platform to the electorate's views by testing alterations with focus groups and speeches. After finitely many refinements, the improved challenger faces the incumbent.

The GAPs, the third type of parties we consider, employ a genetic algorithm to guide their search (see Goldberg 1989; Holland 1975). Instead of adapting a single position, genetic algorithms adapt a population of platforms, attempting to discover nonlinear (epistatic) interactions among variables. Genetic algorithms were designed to work well in "complex" environments—spaces with nonlinearities, discontinuities, noise, and high dimensionality. For our purposes, GAPs provide an indication vis-à-vis the other algorithms of the inherent difficulty of the spatial election problem.

The GAPs represent parties whose potential candidates shift positions both by borrowing from competitors and by testing their own alterations. The genetic algorithm generates new platforms using three procedures. It begins with the random creation of, say, 12 platforms. The first operator, reproduction, randomly selects (with replacement) 12 pairs of candidates from the list and reproduces only the preferred member of the pair. The resulting candidates are then randomly arranged in pairs to which the cross over operator is applied. During crossover, the candidates randomly decide (with probability 50%) whether or not to trade positions on a few issues. If they decide to switch, they exchange groups of positions. Finally, the mutation operator allows each candidate to alter positions randomly on an issue or two.

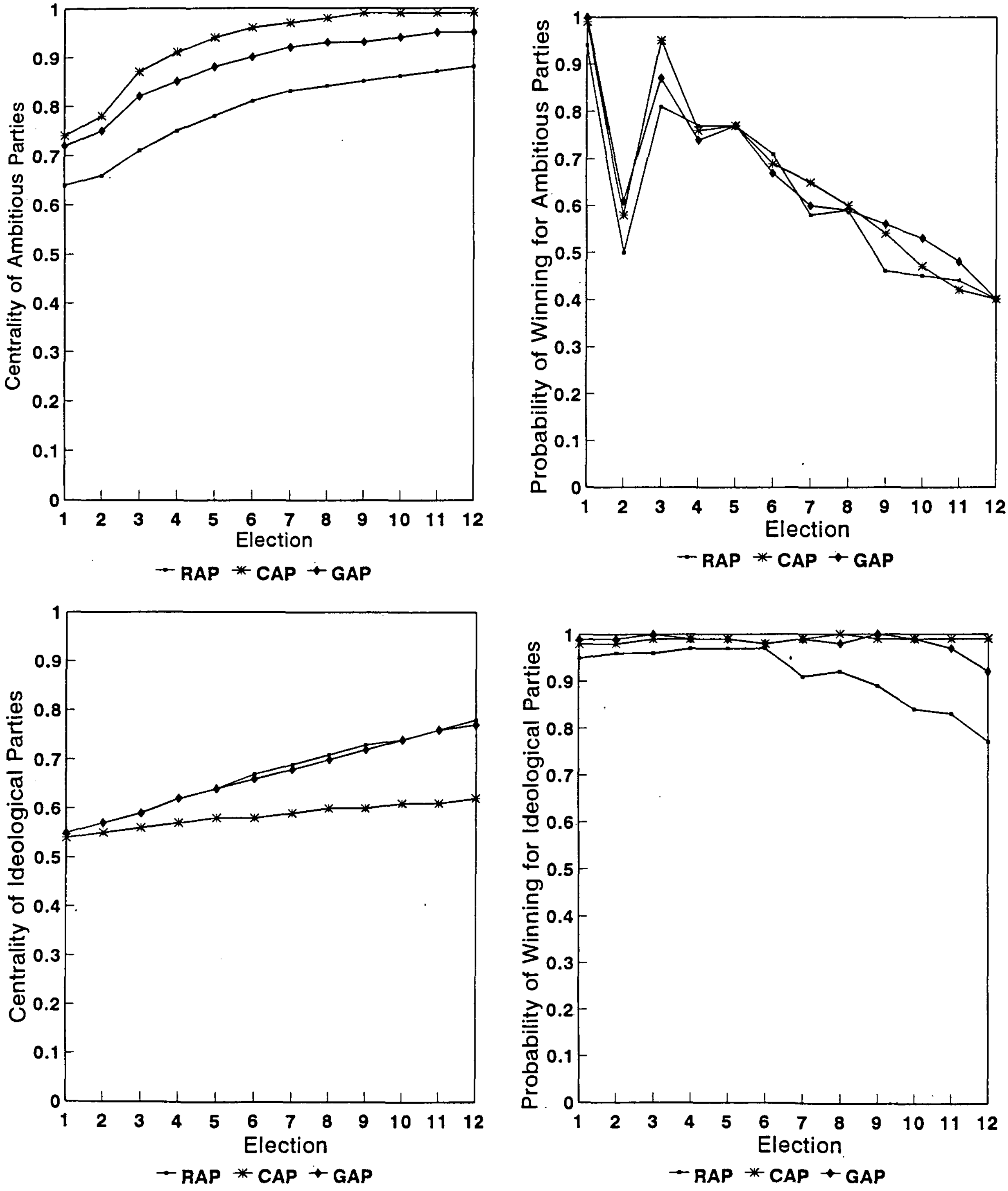
Following biological convention, each application of the reproduction, crossover, and mutation operators is called a *generation*. We count each generation of the genetic algorithm as two units of campaign length, since both crossover and mutation involve candidate platform alterations. At the completion of a campaign of length L, which consists of $L/2$ generations of the genetic algorithm, the party chooses the best-to-date platform.[6]

RESULTS

Several measures are of interest in comparing the different parties and search procedures. Given that our primary concern is the extent to which the distribution of winning party platforms is biased toward regions of high centrality, we record the centrality of winning platforms. We also want to measure the ability of ambitious and ideological parties to defeat the incumbent and to know how far ideological parties must stray from their ideal platforms to do so. Finally, we are concerned with the effect of varying the length of campaigns. The length of campaigns corresponds roughly to the amount of information parties have about voters before an election. Campaigns of length 40 ought to enable parties to compete more effectively against incumbents than campaigns of length 5.

FIGURE 1

Centrality and Success of Adaptive Parties

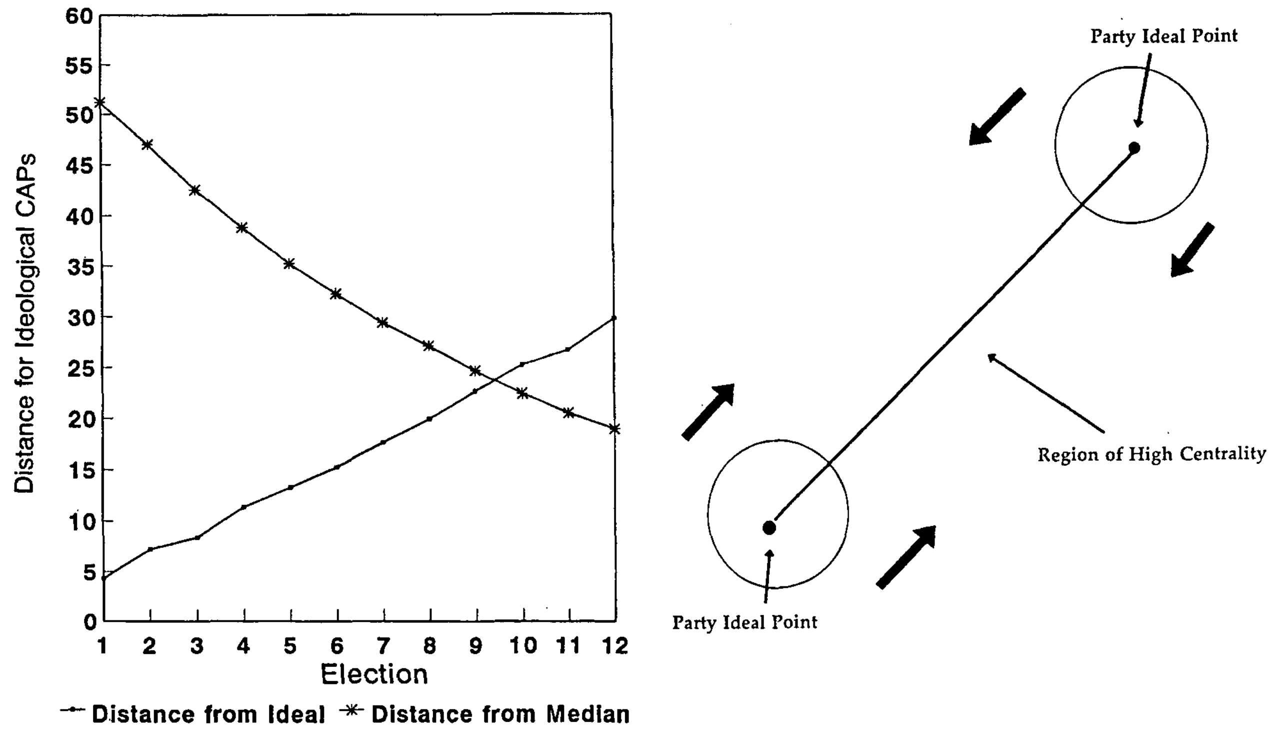


Data represent means over 200 trials

Before presenting our findings, we should note that the robustness of computer simulation results often is sensitive to parameter values. Our findings appear qualitatively invariant to reasonable changes in parameters. We chose parameter values that seem realistic in the study of democratic elections and for

FIGURE 2

Ideological Parties: The Dumbbell Waltz



which a wide range of surrounding values produced similar results. For example, we chose 12 elections because most interesting phenomena were manifest by that time. The following parameters were used:

Voter types (V)	251
Number of issues (n)	15
Positions per issue (k)	7
Strengths (s_{ji})	3
Elections	12
Campaign length (L)	40

All of these fall safely within ranges for which we observed no significant changes in the conclusions.

Centrality values have greater significance when viewed with respect to the distribution of centrality. The Appendix shows numerically estimated distribution and density functions for the centrality of platforms. We compare election outcomes to the cumulative distribution function. Note that a winning party with a platform having a centrality of .55 lies in the upper 17% of the distribution. We ran two hundred trials of a 12-election sequence for each party and type of algorithm. We recorded the centrality of the winning party, the probability that the incumbent would be defeated, and (in the ideological case) the distance to the party's ideal platform.

Ambitious Parties

For all three types of algorithms, ambitious parties moved in directions of higher centrality (see Figure 1). By the sixth election the CAPs and GAPs had average centralities above .9, and the RAPs above .8, which placed all three types of parties in the top .01% of all platforms! It appears that convergence to high centrality and the increase in centrality over time are invariant to the type of search algorithm. As might be expected, the CAPs and GAPs had higher centrality than the RAPs. With respect to the distribution of centrality, however, the differences were not large.

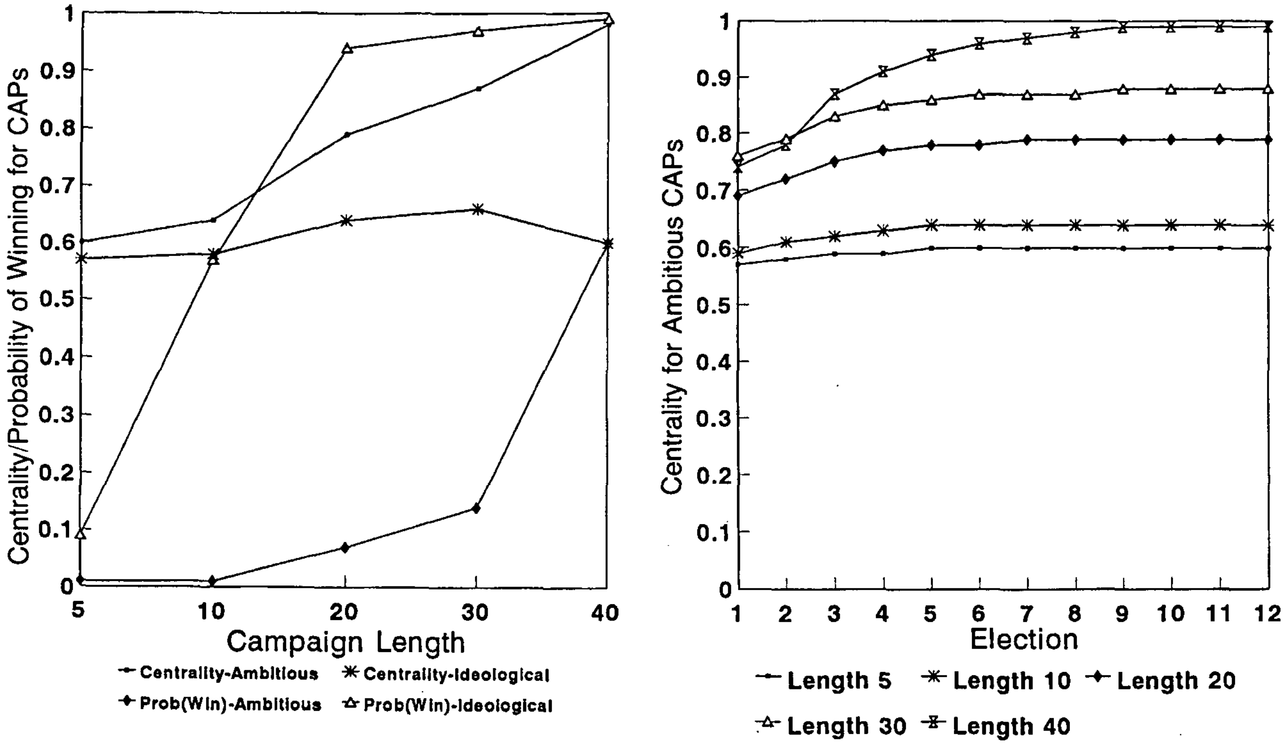
The probability of a challenger's winning decreased from almost 1 in the first election to near .4 by the twelfth election for all three algorithms (see Figure 1).[7] In our model, ambitious challengers have increasing difficulty defeating the incumbent. Incumbency advantage can be attributed to challengers' lack of information, the limitations of adaptive search, and the positions of incumbents' previously adapted platforms.

Ideological Parties

We would expect the centrality of outcomes in elections between ideological parties to be lower than if the parties were solely ambitious. As a consequence, the probability of winning should be much higher in ideological contests. Our results confirmed these expectations. Centralities were lower overall for ideological parties compared with ambitious parties, and there was less variation over elections (Figure 1). Still, all three algorithms were in the top 3% of platforms at the end of 12 elections. Moreover, the probability of

FIGURE 3

Centrality and Success by Campaign Length



200 trials
Values for the 8th election

200 trials

winning was high throughout the sequence of elections, tapering off in the later elections only for the RAPs (Figure 1). Note that the CAPs were not only more likely to win than the RAPs and the GAPs, but they also won with lower centrality. Data reveal the CAPs stayed closer to their ideal points. The CAPs ability to fine tune platforms issue-by-issue generates these results.

Another noteworthy result came from the simulation of ideological parties. We observed that for all three algorithms, the distance to party ideals increased by small amounts, while the distance to the median decreased over time (Figure 2). We refer to this positioning behavior as the *dumbbell waltz*. The challenging party dances in a neighborhood of its ideal platform until it finds a winning platform. This neighborhood slowly converges to areas of high centrality. A chart of winning platforms would consist of two disjoint neighborhoods (one near each of the ideal platforms) and would resemble a dumbbell. The ends of the dumbbell slowly converge as the number of elections increases (Figure 2).

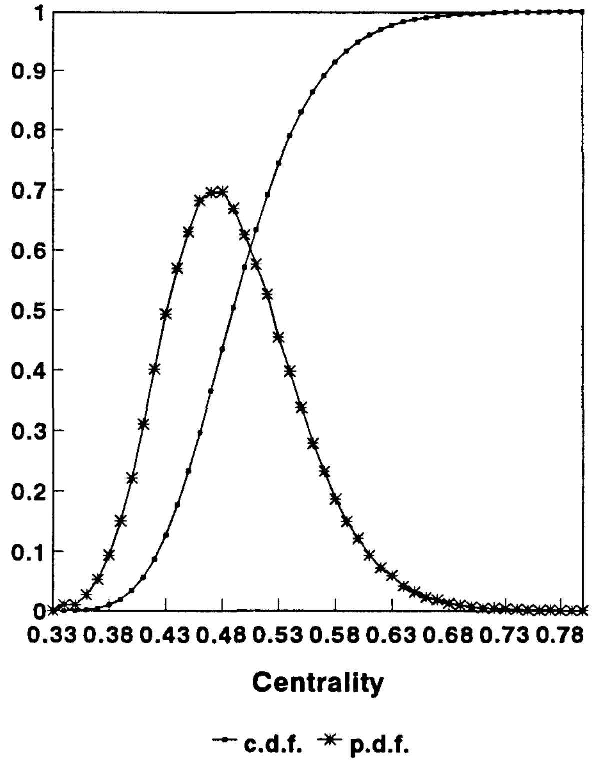
Campaign Length

Increasing the length of the campaign increases the ability of parties to learn about, and adapt to, voters' collective preferences. Figure 3 shows centralities and probabilities of winning for ambitious and ideological CAPs as the length of the campaign increases from 5 to 40. As expected, both centrality and probability of winning tend to increase with campaign length. To give a complete 12-election example, the figure also reveals how increasing campaign length for ambitious CAPs qualitatively increases centrality. For ideological parties, however, centrality varies only slightly with campaign length (see Figure 3) because more informed and intelligent ideological parties are able to locate winning platforms nearer their ideal platforms.

Our results tend to support the idea that boundedly rational parties will converge to central regions of the issue space in a Downsian fashion.[8] Since centrality measures the closeness of parties to voters' ideal points, winning parties with very high centralities can be said to give voters high utility. In this sense, our results suggest that even with fairly simple adaptive parties (e.g., the RAPs), a two-party system should lead to normatively appealing outcomes. Not surprisingly, ambitious parties reach higher centrality than ideological parties. Finally, all three search procedures for both types of parties produced similar outcomes, indicating that there may exist large equivalence classes of adaptive behavior by parties that may allow researchers to undertake a unified analysis. Substantively, our use of three algorithms bol-

FIGURE A-1

Centrality Distribution/Density



Note: 10,000 Monte Carlo trials. c.d.f. = cumulative distribution function; p.d.f. = probability density function

sters the conclusion that departures from the strict informational and rationality assumptions of earlier formal models lead us to convergence results.[9]

DISCUSSION

Rational choice theories of electoral competition have led to two incompatible conclusions. On the one hand, convergence results offer an intuitively appealing explanation for the responsiveness and stability of two-party systems. On the other, chaotic results raise doubts about the legitimacy of democratic outcomes. Both sets of results have relied on strict assumptions of complete information and global optimization. We have relaxed these assumptions about parties and have observed the parties in our model (even ideological ones) converging to central regions of the issue space. Although some mathematical models indicate that democracy might yield arbitrary results with respect to voters' preferences, our model suggests that this typically will not happen with adaptive parties. Moreover, boundedly rational parties may not be able to defeat incumbents. Mathematical proofs of the existence of winning strategies should not be equated with the ability of human actors or organizations to locate them.[10]

Our results, supported by their invariance to parameter choices and search procedures, also help confirm basic intuitions about the role of information and ideologies in elections. As the length of campaigns increases or as parties have more information about voters, parties tend to converge toward centrist outcomes. This observation seems to be consistent with general comparisons of both national and local elections. Extreme candidates rarely emerge as national candidates; and when they do, they lose by a wide margin. Yet at the local level, extremists can thrive. The lack of information among voters has received a great deal of theoretical attention; the lack of information among parties and candidates deserves to be explored, as well.

Our techniques are designed to analyze the behavior of a complex adaptive system, a system in which endogenous aggregate behavior emerges from the knowledge- and rule-based behavior of individual agents. Our model can be extended in many directions, incorporating a variety of components known to exist in the real world. It is possible to include correlated preferences, noisy polls, coadaptive parties, interest groups, issue polling, voters with incomplete information, and multiple parties in future models.[11] With all of these extensions, there is the opportunity to calibrate a simulation to data.

APPENDIX

Figure A-1 shows numerically estimated distribution and density functions for the centrality of platforms.

Notes

Complete data and computer programs are available upon request. We wish to thank Jeffrey Banks, Michael Kirschenheiter, Roger Myerson, Benjamin Page, and Stan Reiter for helpful comments on earlier drafts. The research support of the Center for Mathematical Studies in Economics and Management Science at Northwestern University and the Santa Fe Institute and a grant from Sun Microsystems are gratefully acknowledged.

1. The top cycle set is the smallest set of points whose members defeat all points not in the set. For example, if A defeats B, B defeats C, C defeats A, and each defeats all other points in the space, then {A, B, C} is the top cycle set.

2. By *logical consistency* we mean in the narrow sense of adherence to a rule and not in the broader sense of satisfying mathematical axioms.

3. Central limit theorems and the like are not appropriate given the relatively small number of voters and the size of the space. With 15 issues, 7 positions, and 3 strengths, there are 21^{15} ($\approx 10^{20}$) possible voter types.

4. This construction allows an ideological party to choose a platform that is further from its ideal platform than the incumbent's platform is. However, this only occurs when the distance between party ideal points is improbably small.

5. As an example, in a five-issue, seven-position space, where the current party platform is (6, 2, 5, 4, 1), the party will be much more likely to test the platform (5, 2, 5, 4, 2) than the platform (1, 6, 1, 3, 6), given their relative distances to the current platform.

6. We use a crossover rate of .5; so on average six platform adaptations occur per generation. Therefore, in L/2 generations, the GAPs will test more than L platforms.

7. The dip in the second election results from our random assignment of an incumbent prior to the first election. Half the time, the challenger defeats the incumbent prior to adapting a new platform. After adaptation, the election is a landslide. Subsequently, the exincumbent (now the challenger) has a difficult time winning the second election against such a strong opponent.

8. Kramer (1977), relying on stricter assumptions, has shown that parties will converge toward the minmax set (the set of platforms that lose by a minimal amount) but that the minmax set is not a stable attractor. In other words, parties can leave the minmax set to win once they converge to it. Our results suggest that if movements out of the minmax set occur, they do not, on average, lower centrality.

9. The equivalent performances of the GAPs and the other less sophisticated algorithms also suggest that the underlying search problem is relatively easy.

10. In chess, a finite perfect information game, an optimal strategy exists, yet no one has found it to date.

11. Convergence tends to be robust to changes in the distribution of voters' preferences, although convergence rates may change. For a complete discussion, see Page, Kollman, and Miller (1992).

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REPRESENTATIVE DEMOCRACY AND STATE BUILDING IN THE PROGRESSIVE ERA

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I draw upon state-building and legislative literatures to investigate how constituency-based representative institutions in the Progressive Era nationalized innovative public policies, thereby expanding the authority of the federal government as a component of the modern American state developing at that time. Using state-level referenda votes as measures of grassroots views, multivariate analysis discloses the impact of district opinion, as well as party and district economy, as major determinants of House roll call voting on landmark regulatory legislation authorizing federal intervention in market relationships, state suffrage qualifications, and life-style behaviors involving intoxicating beverages.

The Progressive Era has long been depicted as a responsive "search for order" activated by social, economic, and political problems attending rapid and brutal industrialization at the turn of the century (Burnham 1982, 47; Wiebe 1967). Recently, however, "new institutionalist" approaches point to the expansion of state capacity as the most salient outcome of that search, designating the Progressive Era as a critical time that set parameters for the development of the modern American state (Amenta et al. 1987; Buenker 1977, 57; Ethington n.d.; Orloff 1988; Skocpol 1992; Skowronek 1982).[1]

State capacity grew in two significant ways in the Progressive Era. First, after the turn of the century, more and more observers acknowledged that social and economic ills attributed to an unrestricted capitalistic market system were anything but self-correcting. Consequently, union-backed legislative policies, passed at both state and national levels, thereby expanding the scope of government to include intervention in laissez-faire's hitherto sacrosanct free-contract relationship between business and labor.[2] The power of government regulation also expanded to such new areas as the food and drug industries, as well as development of innovative income tax and fiscal policies (Epstein 1986).[3]

Second, the scope of government authority was not only reconstituted in the Progressive Era at the state and national levels but was transferred from the former to the latter. Previously decentralized state capacities located in dispersed "island communities" became unified and centralized (Wiebe 1967, xiii), producing what Samuel Beer terms a transitional period of dual federalism (1973, 69). How this reconstitution and transference came about, of course, poses one of the more intriguing questions to scholars investigating this period.

Electoral Models of Policy Innovation: Mainsprings and Wellsprings

The realignment thesis provides a powerful explanation accounting for how decentralized electoral opinion can be the "mainspring" of policy innovation at the national level, once it becomes expressed in a cataclysmic, critical election establishing a long-lasting partisan regime shift (Brady 1973, 1984, 1988; Burnham 1970; Key 1955, 1959).[4] Yet this model is not applicable to the 1900–1920 Progressive Era decades, falling (as they do) in the nonrealignment period between the critical election of 1896 and the New Deal.[5]

Others point to the "electoral connection" as the key feature of the American political system (Mayhew 1974) by showing how political elites attend to what we might term the "wellspring" of constituency opinion on an ongoing basis. In this view, citizens continuously have "a form of indirect access to public policy making," exerting pressure both at the point of elections and throughout terms of office (Arnold 1990; Bartels 1991; Ginsberg 1976; Mayhew 1974, 1987; Powell 1982, 658; Walker 1966, 286).[6]

Though scholars typically research the effects of discontinuous realigning elections upon adoption of innovative national policies (Shafer 1991), the state-building effects of continuous electoral processes remain relatively unexplored. Yet in at least one context, we might expect the ongoing electoral connection between local constituents and national elites to have state-building outcomes at the federal level, namely, a stage of policy development in which grassroots issue positions anticipate those to be established at the national level. In such circumstances, national political elites following progressive grassroots policy cues not only represent their constituents but thereby effectively transfer policy innovations from the state level (where they are already established) to the national level. In such a scenario, it is the representative structure of Congress linking it to grassroots constituents as a source of policy innovation that accounts for its institutional role as state-building agent passing legislation which expands federal authority.[7]

The Progressive Era embodies such a context as a time when innovative public policies were widely diffused across states prior to the adoption of similar policies at the national level (Gray 1973; Skocpol

1992). Before 1900, for example, 28 states passed some form of hours laws limiting the working day; and 15 states, including the most industrial, had passed laws specifically regulating hours of work for railroad and streetcar employees. Not surprisingly, in the wake of state-level diffusion of such policies, a comparable program of union-backed labor legislation began to be adopted at the national level after the turn of the century.

Yet while many view our federal system as one in which reform "percolates" up from the local to the state to the national level, few analyze what constitutes the percolation mechanisms.[8] Rather, scholars most often depict innovative public policies established at the national level in nonrealignment periods as a result of national forces alone or partisan sectional cleavages generated solely by economic interests.[9] This project expands such analysis by also assessing how local policy positions influenced the passage of innovative national legislation in the Progressive Era.

POLICY CONGRUENCE AND CONSTITUENCY OPINION

Policy congruence linkages between grassroots constituents and their representatives constitute, for many scholars, the core of democratic theory, empirically defined by the degree of issue concordance between the two. Though the meaning of policy congruence between representatives and their constituents raises complex questions, empirical assessments of how policy opinions of grassroots voters match those of their representatives are relevant to all analyses of representative roles and norms (Bartels 1991, 468, 457; Fiorina 1974; Kingdon 1981).[10] As Robert Weissberg put it succinctly, "Representation means a high correlation between constituency opinion and roll-call voting on a pairwise basis" (1978, 536).

Measuring Constituency Opinion

While roll call votes are readily available as concrete indices of legislators' issue positions in policy congruence research, measurement of constituency issue opinion has proved more vexing (Clausen 1973, 119; Hedlund and Friesema 1974, 399; Jewell 1985; Miller and Stokes 1963, 47). Warren Miller and Donald Stokes (1963) use direct, attitudinal survey measures of constituents' issue preferences in their classic study of policy congruence in the United States in the late 1950s. Philip Converse and Roy Pierce (1986) replicate this design in their study of France in the late 1960s and early 1970s. While both studies provide direct measures of constituency opinion, they also illustrate problems endemic to the use of attitudes as indices of policy positions.[11]

Yet, for historical research, the most seriously limiting feature of attitudinal surveys is the lack of them. The first systematic public opinion polling did not commence until the 1930s, rendering earlier periods, such as the Progressive Era, barren of attitudinal data. Clearly, researchers interested in the early twentieth century must turn elsewhere for direct evidence of grassroots opinion.

Referenda Votes

A technique advocated by contemporary researchers having potent utility for research on historical periods is the use of state-level referenda votes as direct measures of constituency issue positions. In some ways, referenda votes are the more preferred index of constituency opinion. They are discrete, behavioral legislative choices made by grassroots voters on specific policies and thus inherently more comparable to the roll call votes of legislators than the responses to survey questions can be.[12] Furthermore, their use in contemporary and historical studies of state legislative policy outcomes is impressive (Arneson 1927; Campbell 1977; Crane 1960; Dykstra and Hahn 1968; Erikson, Luttbeg, and Holloway 1975; Hedlund and Friesema 1974; Kuklinski 1977, 1978).

More recently, referenda votes have been used to study policy connections operating between state and national levels. L. Marvin Overby (1991), for example, used the returns of state referenda on nuclear freeze policies to show that constituency opinion affected House roll call voting on similar legislation in the 1980s. This resourceful application of referenda votes, nevertheless, failed to aggregate returns by congressional districts, using, instead, only state-level totals.

I use referenda votes, aggregated by congressional districts, as a direct measure of constituency opinion to investigate how policy congruence operated in the Progressive Era between grassroots voters and their representatives in the House. In addition to demonstrating the utility of referenda votes, I push a study of policy congruence back in time to key historical decades, illuminating how policy congruence contributed to the centralization of state authority occurring in the early twentieth century.[13]

DATA AND METHODOLOGY

To develop a measure of district-level opinion on union–labor, woman suffrage, and prohibition policies, I collected as many county-level state votes on referenda and initiatives as I could find by means of an extensive two-year search of state yearbooks found in the libraries of Harvard, Michigan, and Minnesota universities, the Library of Congress, uncoded data files at the University of Michigan Inter-University Consortium for Political and Social Research (ICPSR), and correspondence with many state archive facilities. I expanded the data set for labor to include more states by using state legislature votes as proxies for district referenda returns.[14] All data collected were at the county level, which I recoded to the district-level on the basis of the *Official Congressional Directory*, which lists congressional districts by House member and county for every Congress.

The resulting data set, which includes most of the country on at least one issue, comprises a measure of grassroots opinion for 244 districts on union–labor issues, 241 districts on woman suffrage, and 101 districts on Prohibition.[15] I pooled local union–labor votes covering a range of issues, such as workmen's compensation, hours laws, the right to picket, minimum number on railroad crews, and minimum wage and conducted several tests to be sure these policies all fell on the same union–labor domain.[16] I also pooled district-level votes on woman suffrage and prohibition and developed a longitudinal measure of woman suffrage to examine how shifts at the district level over time correspond with shifts at the national level resulting in the 1919 passage of the amendment.[17]

District opinion is measured two ways: as a continuous percentage (percent voting in favor of an issue) and as a dichotomous measure defined as "oppose" (0–49% support) and "favor" (50–100% support). The latter is used in logistic regression analysis for scale comparability with other dichotomous variables.[18]

The Sixty-Third (1913–15) and Sixty-Fourth (1915–17) congresses passed landmark labor bills. I chose to analyze legislation that (1) was decided by a roll call vote, (2) had at least a minimal level of opposition (5%), and (3) was clearly labor in content, verified by reference to debates in the *Congressional Record* and historical scholarship.[19] In cases where several votes were taken on the same bill, I chose the votes most closely allied with labor interests, as determined by historical and *Congressional Record* sources. The roll call vote I analyze for the Clayton Act, for example, is the first version of this bill voted on by the House, since this was the version most actively backed by labor organizers, rather than the final compromise bill voted on after it had been sent to the Senate and returned to the House (Link 1956, 427–31). Additional labor legislation chosen for analysis on this basis are the vocational education bill and a labor dispute fund bill voted on in the Sixty-Third Congress.

In the Sixty-Fourth Congress I analyze the landmark vote on the Adamson Eight-Hour Day Act and the vote taken just prior to its passage on a procedural tactic related to its passage (1916); the first, 1916 vote on the Immigration Act requiring a literacy test for entry into the United States, rather than the vote taken in 1917 overriding Wilson's veto;[20] and unemployment legislation. I develop four labor scales out of this total collection of labor votes by cumulatively adding the number of supporting votes cast by House members on each of these scales to determine members' scale scores, as described below.

All House roll call votes on union–labor, woman suffrage, and Prohibition policies are coded at the House level as 0 for *nay* and *paired nay* votes and 1 for *yea* and *paired yea* votes; missing votes are omitted from the analysis. (The use of masculine pronouns when discussing representatives in the Sixty-Third and Sixty-Fourth congresses reflects the fact that no women had been elected to the House at this time.)

I use indices originally developed by Richard Bensel (1984) to code district economy as 1 for core districts ($300 or more value added per capita in manufacturing), 2 for diverse districts ($200–299) and 3 for periphery districts (less than $200).[21] I code party of House member as 1 for Democrats and 0 for Republicans. Policy congruence between House members and their constituents is analyzed using correlational analysis, cross tabulation, and logistic and linear regression analysis.

ANALYSIS AND FINDINGS

Let us begin by examining the impact of district opinion, House partisanship, and district economy on roll call voting on three key legislative policies debated in the Sixty-Third Congress: the Clayton Act, which passed with a strongly supporting vote of 83.4%; the Woman Suffrage Amendment, which failed to garner even a majority vote; and the Prohibition Amendment, which a bare majority in the House supported (52.3%).

Union–Labor: The Clayton Act

Passage of the Clayton Act in 1914 was a jewel of an accomplishment for the new presidential administration of Woodrow Wilson. Hailed by Samuel Gompers as "labor's magna carta" (quoted in Link 1956, 432), this legislation not only put in place moderately strong antitrust regulations but also supported the right of union workers to strike, thereby countering antilabor Supreme Court interpretations of the Sherman Anti-Trust Act (Sanders 1986). Democratic forces in the House backed the Clayton Act to the hilt, going so far as to invoke a binding caucus vote. The result was that over 95% of the Democrats voted for the Clayton Act compared to only 44% of the Republicans, leading some to the obvious conclusion that national-level partisan dynamics backed by executive leadership were the primary causes responsible for its passage.

According to our electoral model of policy innovation, however, constituency opinions on union–labor legislation should also figure prominently as influences determining the outcome of roll call votes on the Clayton Act. With our direct measure of grassroots opinion, we can investigate this possibility. Indeed, as indicated by the logistic regression analysis presented in Table 1 for Model 1, district opinion was clearly a powerful force contributing to the success of the Clayton Act. District opinion *alone* correctly predicts nearly 80% of the roll call votes on the Clayton Act. Even more telling, the addition of partisanship in Model 2 by no means negates the influence of district opinion; it remains virtually as strong a predictor as party. (Logit coefficients are 2.071 and 2.231, respectively, the positive sign for party indicating greater Democratic support.)

TABLE 1

Logit Coefficients for House Support for Clayton Act, Sixty-Third Congress, 1913–15

INDEPENDENT VARIABLES	HOUSE ROLL CALL VOTE ON CLAYTON ACT[a]		
	MODEL 1	MODEL 2	MODEL 3
District opinion[b]	1.896 (.599)**	2.071[c] (1.065)**	2.080 (.998)**
Party of House member[d]	—	2.231 (.333)***	2.268 (.347)***
District economy[e]	—	—	1.230 (.453)**
Intercept	−2.336 (1.155)**	−2.455 (2.048)	−4.208 (.638)**
Correctly predicted (%)	79.41	87.96	90.05

Note: Figures are unstandardized logit coefficients; standard errors are in parentheses.

[a] House roll call votes coded 0 for nay, paired-nay, 1 for yea, paired-yea.

[b] For greater comparability with dichotomous variables, district issue vote coded 1 for 0–49%, 2 for 50–100%.

[c] Significance level calculated by computer program reported at .05 level; calculation based on truncated printed output falls slightly under requisite 1.96 z-score.

[d] House member's party coded 1 for Democrat, 0 for Republican.

[e] District economy coded 1 for core ($300 or more value added per capita in manufacturing), 2 for diverse ($200–$299), 3 for periphery (<$200); codes made available by Elizabeth Sanders.

**p = .05.

***p = .001.

By adding constituency opinion, therefore, we gain a more complete understanding of how the Clayton Act passed in the House with such an impressive majority. Not only was the Democratic party in control, having won 69% of the seats relative to 32% for the Republicans, but also grassroots opinion was strongly behind union–labor legislation. Support for union policies had diffused widely across states by 1914, reflected in an overall district mean support of 80.66 (McDonagh 1992). It was the duality, therefore, of both broad-based favorable grassroots sentiment and such national forces as the Democratic caucus and executive branch advocacy that produced winning margins for the Clayton Act.

Uncovering the strong relationship between constituency opinion and roll call voting on the partisan-backed Clayton Act raises another intriguing question: How might district opinion be involved in establishing party discipline operating on the passage of this controversial bill? Specifically, could broad-based constituency support for union–labor policies be one reason why Democrats made the Clayton Act a binding caucus vote in the first place?

I cannot investigate directly how constituency opinion may have influenced the Democratic caucus without access to its notes recording the roll call tally on this decision. It is relevant, however, that 87.8% of the Democrats in my sample were elected by districts registering strong support for union policies and that the overall district mean support for union–labor policies was 10 points higher for Democrats than Republicans (83.96 and 73.80, respectively, a statistically significant difference at $p = .05$; see McDonagh 1992).

For most Democrats, therefore, supporting the Clayton Act posed no conflict between constituency and party; rather, the dual congruency with both was most likely mutually reinforcing. It is not unreasonable, therefore, to assume that constituency influence—although empirically most easily measured in terms of its congruence with roll call votes (Kingdon 1981; Overby 1991)—undoubtedly had additional influences on informal decision-making processes responsible for the very visible partisan cleavages characterizing this bill (Holt 1967; Jones 1970; Link 1956).

Elizabeth Sanders (1986) points to the influence of level of industrialization on House roll call voting on the Clayton Act, which contained an antitrust component traditionally important to farmers, as well as the right to strike component appealing to labor interests. She analyzes how a farmer–labor alliance coalesced around its support, reflected in the disproportionate tendency of House members from rural, nonmanufacturing districts to support the measure compared to those who were elected from core districts where manufacturing interests usually electorally dominated those of labor (1986, pp. 144–45).

Looking at the relative impact of district, party, and district economy, therefore, broadens our understanding of influences upon passage of the Clayton Act, as reported in Table 1 for Model 3. District opinion clearly remains a distinct and powerful influence, even with the inclusion of party and district economy. (Logit coefficients are 2.080, 2.268, and 1.230, respectively.) Although we know that some northern Democrats and Republicans representing labor constituencies voted for the Clayton Act (Sanders 1986), the positive sign for district economy indicates that relatively greater support stems, in general, from periphery districts (as we would expect).

Analyzing votes *incongruent* with partisan and/or district economic interests also reveals the impact of district opinion. Republican House members representing core districts generally should be expected to vote against the Clayton Act. Yet, a small minority (19%) cast yea votes, and *all* of these incongruent votes are from Republicans representing districts supportive of progressive labor policies. For some, though not all, Republicans, prolabor constituents overrode both party and economic interests combined.

Similarly, despite their nearly unanimous support of the Clayton Act, a few Democrats voted against it; and these incongruent votes were cast by Democrats representing districts expressing a negative stance on union–labor issues, reflected in the probability of their yea vote dropping from .969 to .798 as we shift from "favorable" to "unfavorable" districts on labor policy. Similarly, Republicans from diverse districts

TABLE 2

Regression Coefficients for House Roll Calls on Labor Scales, Sixty-Third and Sixty-Fourth Congresses, 1913–17

INDEPENDENT VARIABLE	CONGRESS LABOR SCALES[a]			
	SCALE 1 (63D)[b]	SCALE 2 (64TH)[c]	SCALE 3 (63D–64TH)[d]	SCALE 4 (63D–64TH)[e]
District opinion[f]	.604 (.220)**	1.332** (.624)	1.903* (1.109)	1.360** (.447)
Party of House member[g]	1.537*** (.072)	1.659*** (.112)	3.099*** (.168)	1.171*** (.106)
District economy[h]	.031* (.016)	.147*** (.025)	.170*** (.040)	.115*** (.023)
Constant	−.089 (.219)	−.293 (.621)	−.679 (1.112)	−.288 (.442)
Adjusted R^2	.668	.569	.682	.551

Note: Figures are unstandardized regression coefficients; standard errors are in parentheses.
[a] House roll call votes coded 1 for yea and paired-yea, 0 for nay and paired-nay.
[b] Scale 1 is additive number of yea votes cast for Clayton Act, vocational education, and labor dispute fund legislation for House members voting on at least 50% of scale items, including Clayton Act.
[c] Scale 2 is additive number of yea votes cast for two votes on the Adamson Act, immigration restriction, and unemployment legislation for House members voting on at least 50% of scale items, including Adamson Act.
[d] Scale 3 is additive number of yea votes cast for combined scales 1 and 2 items for House members voting on at least 50% of all scaled votes, including at least one of the "Big Three" (Clayton, Adamson, immigration restriction).
[e] Scale 4 is additive number of yea votes cast for Big Three for House members voting on at least Clayton and Adamson Acts.
[f] District Opinion coded 1 of 50%–100% favor union labor legislation, 0 if less than 50% favor.
[g] Party of House member is coded 1 for Democrat, 0 for Republican.
[h] District economy is coded 1 ($300 or more value added manufacturing), 2 for diverse ($200–$299), 3 (<$200); codes made available by Elizabeth Sanders.
*p = .10.
**p = .05.
***p = .001.

opposing labor policies are almost certain to vote nay, while those representing districts supportive of labor are more likely to vote yea.[22] These findings confirm, therefore, the independent influence of district opinion in multivariate models including partisanship and district economy.

Union Labor Scales: The Sixty-Third and Sixty-Fourth Congresses, 1913–17

Policy congruence research often employs scales measuring what Heinz Eulau terms "policy dispositions," rather than discrete roll call votes (Allen, Clausen, and Clubb 1971; Bartels 1991, 462–63; Clausen 1973; Eulau 1987; Olssen 1980). To expand analysis, I constructed four labor scales for the Sixty-Third and Sixty-Fourth Congresses: (1) a scale composed of votes on the Clayton Act, vocational education, and appropriations for a labor dispute fund (Sixty-Third Congress); (2) a scale composed of votes on the Adamson Eight Hour Day Act, passage of the immigration restriction bill, and unemployment insurance legislation (Sixty-Fourth Congress); (3) scales 1 and 2 combined; and (4) a scale of major pieces of legislation passed in either the Sixty-Third or Sixty-Fourth Congress (Clayton, Adamson, and immigration restriction bills).[23] A House member's scale score is the additive number of yea votes cast on a scale.[24]

Analysis of scaled votes show district opinion and party to be the primary determinants of labor support in the Sixty-Third and Sixty-Fourth Congresses (Table 2). Even when scales include a binding party vote (e.g., scales 1 and 3), the impact of district support remains strong. District opinion and party logistic coefficients approach parity as influences accounting for labor support (1.332 and 1.659, respectively), measured by scale 2 for the Sixty-Fourth Congress, again confirming the impact of district opinion for passage of national legislation.

Scales combining labor votes in both the Sixty-Third and Sixty-Fourth congresses (scales 3 and 4) also show the influence of constituency sentiment. District opinion is two-thirds that of party (1.903 and 3.099, respectively) as a predictor of House roll call voting on scale 3; and its impact on scale 4 (combining the Clayton, Adamson, and immigration restriction votes—the "Big Three") slightly exceeds the potency of party (1.360 and 1.171, respectively). Analysis of scaled labor votes, therefore, conforms to the pattern evident for individual labor votes, confirming the effect of district opinion as a significant influence upon House roll call voting on progressive labor policies at the national level.

Woman Suffrage and Prohibition

The woman suffrage issue has a fascinating legislative history (Flexner 1975). First introduced into Congress as the Susan B. Anthony Amendment in the aftermath of the Civil War in 1868, it was not until 1915, in

TABLE 3

Logit Coefficients for House Support for Woman Suffrage and Prohibition, Sixty-Third Congress, 1913–15

	HOUSE ROLL CALL VOTES[a]	
VARIABLE	WOMAN SUFFRAGE	PROHIBITION
Model 1		
District opinion[b]	1.518 (.414)***	1.404 (.449)**
Intercept	−1.203 (.501)**	−1.943 (.736)**
Correctly predicted (%)	65.57	67.39
Model 2		
District opinion[b]	1.250 (.429)**	1.784 (.583)**
Party of House member[c]	−.140 (.159)	−1.419 (.356)***
District economy[d]	.664 (.212)**	.992 (.384)**
Intercept	−2.017 (.601)***	−4.259 (1.336)***
Correctly predicted (%)	65.50	76.47

Note: Figures are unstandardized logit coefficients; standard errors are in parentheses.
[a] Roll call votes coded 0 for nay, paired-nay; 1 for yea and paired-yea.
[b] For comparability with dichotomous variables, district issue vote coded 1 for (0%–49%), 2 for (50%–100%).
[c] Party of House member coded 1 for Democrat, 0 for Republican.
[d] District economy coded 1 for core ($300 or more value added per capita in manufacturing), 2 for diverse ($200–$299), 3 for periphery (<$200); codes made available by Elizabeth Sanders.
$^{**}p = .05$.
$^{***}p = .001$.

the Sixty-Third Congress, that the first roll call vote on this legislation was held in both the House and Senate. Another amendment also came up for consideration, namely, Prohibition. I examine the impact of district opinion as a sole explanatory variable, as well as the relative impact of district opinion, partisanship, and district economy on roll call votes for woman suffrage and Prohibition in Table 3.[25]

At the outset, it is clear that grassroots opinion is strongly related to both woman suffrage and Prohibition. (Logit coefficients are 1.518 and 1.404 and correctly predict 65.57% and 67.39% of the roll call votes, respectively, as reported in Table 3 for Model 1.) In addition, district opinion remains the strongest relative effect when partisanship and district economy are added (Model 2). For woman suffrage, the logit coefficient for district opinion is 1.250 compared to only −.140 and .664 for partisanship and district economy, respectively; and for Prohibition, the corresponding logit coefficients are 1.784 for district opinion compared to −1.419 and .992 for partisanship and district economy, respectively.

On the basis of the logistic regression models reported in Table 3, we can better understand the failure of the Woman Suffrage Amendment to garner even a majority of votes in 1915. District opinion was virtually the only influence and it was decidedly negative on the suffrage issue. The overall district mean level was a low 44.3% (McDonagh 1992). Little wonder that this legislation went down to such a conclusive defeat on its first House roll call vote in 1915.

The Prohibition Amendment deviates somewhat from the woman suffrage pattern (Table 3). Although district opinion is the strongest effect with a logit coefficient of 1.784, party is also a major influence, evident by the logit coefficient of −1.419 (the negative sign indicating greater Republican support). Analysis of this multivariate model, however, also helps account for the failure to generate the two-thirds majority vote necessary for passage of a constitutional amendment. Constituency opinion, the strongest effect, is insufficient for passage. The overall mean district support for prohibition was only 50.8%; and though Republicans were more likely to support prohibition, they were not the majority party in the Sixty-Third Congress, constituting less than a third of the membership compared to Democrats.[26] Without either broad-based district support or backing from the majority party in the House, we can see why this measure also failed in the Sixty-Third Congress.

Nationalizing the Electorate: Woman Suffrage, 1915–19

Though only 47.6% in the House voted to support the woman suffrage amendment in 1915, just four years later this same legislation received a yea vote from over 77% of House members. Though changes in national legislative policy often are attributed to turnover in House membership (Bartels 1991, 458), the intriguing question from the vantage point of this research is the degree to which the dramatic increase in House support between 1915 and 1919 rests upon a concomitant increase in grassroots sentiment over the same period.

House support patterns, indeed, impressively paralleled those at the district level from 1915 to 1919 (Table 4). While only 15.8% of all House districts supported woman suffrage in 1915, four years later, this figure had climbed to a staggering 57.9%, even while the degree of association between district opinion and House roll call voting on woman suffrage remained virtually constant (.533 in 1915 and .526 in 1919).[27] It was not the case, therefore, that the strength of the linkage itself between district opinion and House voting had increased in the intervening years. Rather, it was that suffrage support had diffused broadly across districts between 1915 and 1919, thereby providing a sufficient grassroots base for nationalization of this policy by 1919.

Though the story of how and why horizontal state-level suffrage diffused between 1915 and 1919 begs to be told,[28] our focus here is the vertical connection between the effect of this local diffusion on passage of national legislation. Specifically, how are longitudinal, diffusion patterns of constituency

TABLE 4

Longitudinal House Support for Woman Suffrage, Sixty-Third to Sixty-Sixth Congress, 1915–19

CONGRUENCY AND SUPPORT	SIXTY-THIRD CONGRESS (1915)[a]	SIXTY-SIXTH CONGRESS (1919)[b]
District opinion and House vote[c]	.533[d]***	.526***
House vote congruent with district opinion (%)	67.2	76.5
Districts supporting woman suffrage (%)	15.8	57.9
House vote supporting woman suffrage (%)	47.6	77.3

[a]First roll call vote on woman suffrage.
[b]Final roll call vote on woman suffrage.
[c]District opinion is the percentage voting for woman suffrage; state-level woman suffrage is used when necessary as a proxy for district measure. House roll call vote coded 1 for nay and paired-nay, 2 for yea and paired-yea.
[d]Correlation coefficient (*r*).
****p* = .001.

support for woman suffrage linked to voting shifts on this issue in the House? To answer this question, I categorized districts as follows: those supporting woman suffrage as early as 1915 are *stable yes* districts; those converting to suffrage support by 1919 are *conversion yes*; and those remaining negative on the issue through 1919 are *stable no* districts.

I categorized House members in a similar way. Those supporting woman suffrage as early as 1915 are *stable yea* members; those converting to a yea vote by 1919 are *conversion yea*; those who remain opposed through 1919 are *stable nay*; and new House members casting a vote for the first time in 1919 are categorized either as *replacement yea* or *replacement nay* members, depending on their voting orientation.

We see clear evidence that longitudinal district patterns on woman suffrage correspond to longitudinal House voting patterns (Table 5). Members with either stable yea or stable nay voting pattern are more influenced by districts also exhibiting stable opinion than they are by conversion districts. (Logit coefficients are 4.346 and 2.398, respectively, as shown in column 1.)[29]

Similarly, House members who converted to a prosuffrage position by 1919 are most effected by conversion districts, as indicated by the size of the logit coefficient of 2.275 in Table 5, column 2. Stable antisuffrage districts had negative effects on House conversion to a prosuffrage stance (as we would expect), indicated by the size of the constant term (−1.625). The lack of cases of House members converting from stable yes districts most likely occurs because House members from such districts vote yea in the first place (column 1), thereby making options to convert to prosuffrage moot.[30]

Turning to replacement members, we see that the greatest effect stems from districts with stable patterns of support, as indicated by the logit coefficient of 9.423.[31] Districts with a longitudinal pattern of stable suffrage support produce yea votes virtually

TABLE 5

Logit Coefficients for Longitudinal House Support for Woman Suffrage, Sixty-Third to Sixty-Fourth Congress, 1913–21

	LONGITUDINAL HOUSE SUPPORT FOR WOMAN SUFFRAGE[a]		
DISTRICT OPINION TYPOLOGY[b]	STABLE MEMBERS[c]	CONVERSION MEMBERS[d]	REPLACEMENT MEMBERS[e]
Stable support district[f]	4.346*** (1.029)	—[g]	9.423 (39.841)
Conversion support district[h]	2.398*** (.368)	2.275*** (.468)	3.297** (1.072)
Intercept[i]	−.476** (.199)	−1.625*** (.303)	.780** (.364)
Correctly predicted (%)	78.40	78.07	89.29

Note: Figures are unstandardized logit coefficients; standard errors are in parentheses.
[a]House roll call votes coded 1 for yea and paired-yea, 0 for nay and paired-nay.
[b]District opinion defined as opposition = 0%–49%, support = 50%–100% State-level woman suffrage used when necessary as proxy for district measure.
[c]Stable House voters are those voting yea or nay consistently, 63d–66th Congresses.
[d]Conversion House voters are those converting from a nay to yea vote by 66th Congress; for analysis, they are grouped with contrasting stable nay House voters.
[e]Replacement House voters are those voting yea or nay for first time in 66th Congress.
[f]Stable support district is one consistently supporting woman suffrage, 1913–19.
[g]Insufficient case for analysis.
[h]Conversion support district is one opposed in 1915 but converting to support for woman suffrage by 1919.
[i]Intercept term is omitted stable opposed district, defined as a district opposed to woman suffrage, 1913–19.
***p* = .05.
****p* = .001.

without exception among House replacement members; and (as we would expect) districts more recently converting to a prosuffrage position also have a positive influence, though not quite as large, evident by the logit coefficient of 3.297. We find confirmation, therefore, that longitudinal constituency support patterns—rather than membership turnover alone—best account for House members' voting patterns on woman suffrage in 1919.

CONCLUSION: CONGRESS AND AMERICAN STATE DEVELOPMENT

Woodrow Wilson viewed Congress as the "central and predominant power" of our federal system (1956, 23); and contemporary research on Congress and legislative processes is a thriving academic industry.[32] Attention to how Congress may effect the operation of the larger political system, however, is less typical; and although the institutional "development of Congress" is a vital area of productive scholarship (Polsby 1968; Price 1971, 1975), development of the American political system itself as a function of congressional lawmaking is a much less common research focus.[33]

One reason why Congress as an institution has been less featured than, say, courts, parties, bureaucracies, interest groups, and the presidency is that scholarship on the Progressive Era customarily analyzes key legislative processes horizontally at either the state (Gray 1973; Skocpol 1992; Valelly 1989) or national level but not at the vertical nexus between the two. Yet it is the representative structure of Congress linking decentralized and centralized loci of political power on an ongoing basis that most distinguishes it from other national-level institutions in our constitutional system (Keller 1985, 1).

Drawing attention to vertical connections between grassroots opinion and national policy elites expands our understanding of how representative processes in the context of the Progressive Era contributed to the development of state capacity at the federal level during that period. By assessing the constituency bases of congressional decision making, we see how House members predisposed to vote congruently with their progressive constituents effectively transferred innovative legislation (once it was diffused across a decentralized, local level) to a centralized national one. As a result, Congress passed landmark regulatory legislation authorizing federal intervention in market relationships, state suffrage qualifications, and life-style behaviors involving intoxicating beverages.

Passage of highly partisan, union–labor measures, for example, depended upon widely diffused labor support at the constituency level, in addition to party and economic interests. Constituency opinion also emerged as a major determinant for a variety of House union–labor scales constructed for the Sixty-Third and Sixty-Fourth Congresses, including such pivotal legislative accomplishments as the Clayton Act, the Adamson Eight Hour Day Act, and immigration restriction.

Nationalizing the Electorate

We also discovered that longitudinal patterns of grassroots opinion, rather than merely turnover in House membership alone, were the most decisive influences accounting for the dramatic rise in House support for woman suffrage from 1915 to 1919. In addition to providing a striking example of the power of grassroots opinion for influencing passage of national legislation, the inclusion of civil rights issues in our analysis reveals new insights about the status of the electorate in state-building models. Though most often depicted as a virtually static feature of American political development (Skowronek 1982, 17–18; Skocpol 1988, 362; Skocpol and Ikenberry 1983, 92), the contested legislative status of woman suffrage suggests, instead, that the electorate is best conceptualized not as a condition of political development taking place at this time but, rather, as an object of that development.

Women's contested voting status in the Progressive Era, far from idiosyncratic, typifies legislation of this period wrestling with new definitions of which groups in the American polity are to have access to formal political power in the form of the vote. The major difference is that women succeeded in winning suffrage guarantees. By contrast, African–American men lost their right to vote on a de facto basis in this period, when, by 1908, every southern state had in place disenfranchising voting laws (Burnham 1987, 29; Kousser 1974). Similarly, the voting of immigrants in the urban North was depressed as a de facto result of "good government" legislation establishing registration and voting regulations (McCormick 1979, 295; Piven and Cloward 1988).[34]

Metaphors and Models of Development: Bulldozers, Laboratories or Coffee Pots?

The research presented here, therefore, suggests yet another metaphor (if not a model) to capture the character of the political development taking place in the Progressive Era in the domain of legislative innovation. In one of the most influential explanations of how the modern American state was built, Stephen Skowronek makes the case that it was necessary to dismantle decentralized loci of administrative state capacity in order to establish in their place strong, centralized agencies. In what might be characterized as a "bulldozer" model of political development, national government authority and offices were "*insulated from the people at large,* [and] governmental forms and procedures necessary for securing order in industrial America emerged through a labored exercise in *creative destruction* . . . by an *extended assault* on the previously established governmental order [i.e., decentralized state]" (Skowronek 1982, 9, emphasis added).

Though Skowronek may well be correct about the necessity for decentralized destruction to occur in order for the creation of centralized administrative agencies, surely, as we have seen, the same cannot be said of legislative policies adopted at the national level by Congress. Here, it is evident that passage of innovative legislation rests upon broadly based grassroots support. Without that support, national legislation fails (as we saw in the case of woman suffrage and Prohibition in the Sixty-Third Congress). With that constituency support, sweeping national legislative changes are possible—as we saw in the case of passage of union labor bills (where grassroot support was already widely diffused by 1914) and woman suffrage once prosuffrage opinion had spread across sufficient districts.

Another metaphor is needed, therefore, to portray the legislative domain of American political development. Here, the famous dictum of Justice Louis Brandeis comes to mind, in which he memorialized our federal system as a "laboratory" where a "single courageous State may, if its citizens choose, . . . try novel social and economic experiments without risk to the rest of the country" (*New State Ice Company* v. *Liebmann* 1932, 311).

Yet we must ask, How apt is this? Far from merely offering national political elites a range of legislative innovations from which they may pick and choose, findings here show how constituency opinion, once it is broadly diffused, provides proactive constraints and guides influencing the national adoption of legislative alternatives. Local policies, therefore, are not merely *copied* at the national level but *transferred* to the national level by means of representative institutions drawing on grassroots support for those policies; and this transfer had vital state-building ramifications in the context of the Progressive Era, when local opinion favoring the expansion of state capacity was in advance of national reform.

Furthermore, although I have assessed only direct effects of constituency opinion upon House roll call voting, one can speculate about indirect effects of district opinion also likely to be operating (Overby 1991). It is plausible, for example, that the decision of the Democratic caucus to make the Clayton Act a binding vote rested on Democratic constituencies strongly supportive of the union-labor policies. Similarly, Wilson's backing of this bill is best viewed as the tip of a pyramid of power whose base is grassroots support. His switch to a strongly supportive stance on woman suffrage after grassroots opinion was in place illustrates the same point (Snapp 1975).

Finally, there is the well-worn notion that innovation can "percolate" up from the local to the state to the national level in our federal system. Operationalizing the "coffee pot" that makes that percolation possible identifies district opinion as a source of innovative public policies in the Progressive Era and representative institutions as structures providing effective mechanisms for transferring innovation from the local to the national level. As a result, we see Congress in a new light as a proactive institutional agent in the Progressive Era by virtue of its representative structure using progressive grassroots support to expand legislative federal authority as a key component in the development of the modern American state.

Notes

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1. As Stephen Skowronek puts it, the dramatically accelerated expansion of national administration in the 1930s and again in the 1960s "may seem to dwarf the significance of the state-building episode" of the Progressive Era, but the "course of institutional development during these more recent decades and the governmental problems encountered in these developments are rooted in this turn-of-the-century departure" (1982, 14).

2. While some investigate the leadership characteristics of Progressives (Thelen 1969, 331), others point out that Progressives did not seek to change the free-market system but, rather, to restrict only its most insidious perceived consequences—"externalities," in the language of economists (Beer 1973, 64–67).

3. Some viewed the invention of direct democracy instruments of governance (e.g., the referendum, initiative, recall, direct primaries, and direct election of senators) as means for accomplishing innovative legislative goals (Link and Catton 1967).

4. Brady (1984) carefully delineates distinctions between electoral results and structural effects characterizing realignment periods.

5. Though partisan strength and dominance shifted considerably from Democratic to Republican margins in the 1900–1920 period, no long-lasting reconfiguration of partisanship occurred during this period (Allen and Clubb 1974). David Canon (n.d.), however, does credit the gradual increase in the number of Democrats elected over time as responsible for the erosion of Republican hegemony in the early twentieth century, even in the absence of full-scale realignment.

6. Patricia Hurley cites the importance of the "linkage between election results and congressional output" not only "during the rare periods of critical realignment" but also as a more general phenomenon (1979, 46). The possible relationship between constituency opinions measured here and scaled ideological orientations remains an avenue for future work.

7. William Riker describes this process, in the context of passage of constitutional amendments in the Progressive Era, as one that simply "universalized a situation which a majority of state legislatures had already created" (1955, 468). According to his analysis, therefore, "peripheralized federalism"

was used as a way to increase the strength of "centralized federalism" (468–69).

8. An exception is Amenta and his colleagues, who analyze the 1935 Social Security Act in terms of prior, state-level policies (1987, 139).

9. Those pointing to national forces operating in the Progressive Era often note the power of the presidency along with that of the Democratic caucus ruling the House after the overthrow of Speaker ("Czar") Cannon in 1910 (Blum 1954; Clubb 1963; Holt 1967; Jones 1970; Link and Catton 1967, chap. 5; Noble 1984, 1001; Roady 1951; Silbert 1966; Tulis 1987; see Bensel 1984 and Sanders 1986, 1990a, 1990b, n.d. for insightful analyses of how economic, regional interests influence national roll call voting).

10. The "representative bond" between grassroots voters and elected policy elite officials has been the subject of intense theoretical and empirical concern (Converse and Pierce 1986; Erikson 1978, 526; Eulau 1987, 176; Eulau et al. 1959; Eulau and Karps 1977; Haefele 1972; Pitkin 1967); and many feasible models of the representative bond have been formulated, such as descriptive, trustee, and delegate relationships. While empirical congruence is not identical to representation (Eulau 1987), it remains one of the most fundamental analytical axes for studies of democratic, representative processes, despite measurement difficulties (Converse and Pierce 1986, 564; Jewell 1985).

11. Small samples (necessitated by the expense of public opinion surveys) attenuate coefficients statistically due to means error (Achen 1977, 1978). In addition, a basic incommensurability exists between general psychological attitudes held by constituents and the specificity of behavioral roll call voting acts of legislators to which they are compared (Stone 1979).

12. Referenda votes are preferable to attitudes as measures of constituency opinion because they are behavioral, dichotomous, yea/nay acts on specific legislative issues. For this reason, referenda votes inherently have scales more commensurable with legislators' roll call voting decisions than do attitudes, which are more diffuse and usually measured as ordinal, rather than dichotomous, variables. Also, summing referenda votes across a district yields a central tendency measure—percentage of support—that includes information about the variance, or heterogeneity, in the district. Homogeneous districts will have either a very high or a very low percentage of support, while heterogeneous districts will register support at marginal levels around 50%. The central tendency measure used across a district for attitudinal scales, on the other hand, is most often the "mean district attitude score," which contains no information about level of district variance.

13. David Mayhew makes the telling point that political elites, including those active at the state level responsible for referenda, are the determinants of public opinion (1986, 4–5). However, this study focuses on the relationship between public opinion at the local level and national political elites, drawing upon the research of others to explicate the forces responsible for generating grassroots public opinion in the first place.

14. These votes include state constitutional amendments requiring referenda approval, as well as referendum and initiative votes. States lacking legislative activity on an issue necessarily are excluded from this analysis.

15. Some districts voted on several referenda in the same issue area at the same time, and other districts cast votes on several referenda in the same issue area at different points in time. I did a factor analysis of referenda to be sure they were comparable to each other in issue content, as defined by their loading on the same factor. All issue categorizations of referenda were confirmed by factor analysis. I then chose among referenda, selecting for analysis those closest in time and content to selected House roll call votes. Where several referenda in the same issue domain were held at the same time, I chose referenda similar in content across states to form the pooled district-level measure of constituency opinion. Additional information will be provided on request.

16. I checked state *Proceedings* of the American Federation of Labor (AFL) to be sure votes used were supported by labor at the local level. I did a factor analyses within each state to be sure that all votes identified as union–labor were on the same factor. I also regressed the pooled labor vote index and each state (coded as a dummy variable) on House roll call voting on labor issues to determine whether there might be additional, independent effects stemming from particular state cultures. No state exhibited a statistically significant ($p = .10$) independent regression coefficient on House roll call voting on labor issues.

17. State-level passage of suffrage is used where district-level votes are not available. Districts in 1915 opposed to woman suffrage are coded 0, and those supporting woman suffrage are coded 1; the same convention is used for 1919. Comparing codes in 1915 and 1919 yields information about whether a district maintained a stable position (either supporting or opposition suffrage) or converted from opposition to support by 1919. (No districts converted in the other direction.) Following scholarly convention, a major form of woman suffrage is defined as full, presidential, and primary woman suffrage (Keller 1977, 15–18; Banaszak n.d.). Information about specific votes used in this analysis will be provided on request.

18. Since the size of a logistic regression coefficient can be effected by the scale size of the independent variable, it is preferable, when doing this type of analysis, to keep all independent variables on relatively comparable scales. Since *party* is a dichotomous variable, other independent variables used in logistic regression analysis have been scaled with this reference in mind.

19. Using these criteria, some important labor votes are omitted, such as the seamen's bill protecting sailors on the high seas, which passed in the Sixty-Third Congress in 1913 on the basis of a voice vote, and the Workmen's Compensation Act, which passed in the Sixty-Fourth Congress by such wide margins that there is not enough variation in my sample to analyze it.

20. The assumption is that initial sources of support and opposition may become clouded in the process of a legislative showdown with the executive branch.

21. I extend grateful thanks to Elizabeth Sanders for making available for this research district codes for sectional interests.

22. The probability of a yea vote shifts from .128 to .539 respectively.

23. Together, these Congresses present a good test case for assessing policy congruence effects by introducing the dual conditions of high replacement and significant alteration in partisan strength. Replacement voters in the Sixty-Fourth Congress comprise nearly half my sample (43.1%), reflecting the dramatic drop in Democratic strength in the House from 1913 to 1915. Though still the majority party in 1915, Democrats lost 61 seats, while Republicans gained 69, between the Sixty-Third and Sixty-Fourth Congresses. The 13 Progressive Republicans and one Independent elected to the Sixty-Third Congress are omitted from analysis.

24. House members voting on less than half the scale items are omitted from the analysis of that scale. For each of the four scales, at least one of the votes cast had to include a vote on the Clayton, Adamson, or immigration restriction bills (the "Big Three").

25. Scholars often note the association of woman suffrage with welfare issues in the Progressive Era, bolstered by women's identification with maternal, nurturing values and effectiveness in reshaping social reform policies (Cott 1987; Skocpol 1992). To the extent that labor issues drew upon welfare concerns, we might expect district economic indexes related to labor also to be related to voting patterns on woman suffrage.

26. Republican House support for Prohibition most likely stemmed from its association with rural, midwestern identities also often represented by Republican partisan interests (McDonagh and Price 1985, 423–26).

27. In addition, House members in 1915 and 1919 were

inclined to vote congruently with their districts at about the same rates (67.2% and 76.5%, respectively).

28. See Banaszak n.d.

29. The positive logit coefficient of 2.398 for conversion districts reflects the behavior of House members voting yea on suffrage prior to their districts' conversion to support for suffrage. One might say that such House members correctly anticipated a switch in district support for suffrage.

30. The dependent variable in Table 6, column 2, is defined by House members' either converting to a prosuffrage position by 1919 or stable House members' continuing to vote nay by 1919. Of this group, only one House member represents a district with a stable prosuffrage position, precluding analysis of how stable prosuffrage district opinion might effect conversion versus stable opposition House voting patterns.

31. The high standard error for this coefficient most likely stems from the fact that stable districts both for and against woman suffrage registered high rates of yea voting among House replacement members, though the rate of members from stable districts supporting suffrage was relatively much higher than for those representing stable districts opposing suffrage (100% and 68%, respectively).

32. Writing in 1885, Wilson (1956) attributed the power of Congress to a tightly disciplined committee system. The structural reorganization of the House in 1910, however, points more in the direction of a wider range of factors (e.g., partisan organization, grassroots opinion, and the effects of changing economic interests).

33. New institutionalists focusing on Congress typically use organizational models that draw attention to its administrative operation as an institutional structure, rather than electoral models analyzing connective representational linkages between grassroots constituents and national policy elites (Rohde and Shepsle 1978; Shepsle and Weingast 1981; Weingast 1987). For an insightful review and critique of new institutionalism literature, see March and Olsen 1984.

34. Passage of southern disenfranchisement laws begins in the late nineteenth century and is complete by the early years of the Progressive Era. Congress, though well aware of southern intent to subvert the Fifteenth Amendment through such legislation, nonetheless passes no legislation counteracting southern disenfranchisement in the Progressive Era. As late as 1919, southern representatives boasted about their successful disenfranchisement of African-American men's vote without suffering even verbal reprimands (as reported in the *Congressional Record* for the Sixty-Sixth Congress). In J. Morgan Kousser's view, disenfranchisement is a typical "Progressive reform" (1974, 260).

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SENIORITY IN LEGISLATURES

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We construct a stochastic model of a legislature with an endogenously determined seniority system. We model the behavior of the legislators as well as their constituents as an infinitely repeated divide-the-dollar game. The game has a stationary equilibrium with the property that the legislature imposes on itself a non-trivial seniority system, and that incumbent legislators are always reelected.

Why do legislatures have seniority systems? Why do incumbent legislators tend to be reelected by wide margins? These are questions that have engaged legislative scholars for some time.

On the issue of the incumbency advantage, a large empirical literature has advanced a number of explanations for this effect. Jacobson (1983) gives a good review of this literature. The explanations range from the increased access of incumbents to money and the media (see, e.g., Mayhew 1974a), to the effects of gerrymandering (Erikson 1972; Jacobson 1983, 13–15), to the decline of the party system and consequent increased use of incumbency, rather than party, as a voting cue (Ferejohn 1977), to constituency service and expertise built up by veteran legislators (e.g., Fiorina 1977a, 1977b; Mayhew 1974b).

Although the question of incumbency advantage and its relation to legislative organization have received considerable attention in the empirical literature on Congress, we know of no attempt to see if any of these explanations can be derived from a full-equilibrium, dynamic model. All of these explanations of the incumbency effect are nondynamic, partial equilibrium explanations. In other words, it is not clear that all individuals at all points in time are behaving rationally. For example, the explanations of the incumbency effect in terms of money and the media typically do not explain why voters should be swayed repeatedly by advertising and campaign literature. The explanation based on gerrymandering assumes that voters' behavior can be determined by certain socioeconomic characteristics of the voters, such as party identification, race, sex, income and religion. It ignores the possibility that both voters and candidates may have incentives to alter their behavior based on the new district characteristics. The explanation based on the decline of parties has no well worked-out theory as to why voters should use cues such as party or incumbency in the first place. The explanation based on constituency service has some weaknesses when one considers the timing of voter and candidate decisions. For example, why should voters vote for candidates who have done a lot for them in the past if the voters have already collected the rewards of the candidate's behavior? These models are a rich source of ideas; and some of the ideas could undoubtedly be made part of a consistent theory in which all participants are behaving rationally and timing issues are dealt with explicitly. However, this has not yet been done.[1]

From our perspective, the most interesting observation in this literature is that many of these variables are determined endogenously by the legislature. It has been argued persuasively by Mayhew (1974b) and Fiorina (1977b) that *Congress organizes itself to serve the reelection goals of its members*. Thus, the franking privilege, the specialized committee system, the norm of reciprocity, and so on are all seen as ways in which Congress advances the reelection goals of its members. Fiorina has taken this argument to its extreme in his thesis that big government is partially a result of the fact that members of Congress benefit from the increased opportunities to intervene in the bureaucracy on the behalf of their constituents.

We shall consider one particular aspect of legislative organization, namely, the seniority system, and build a theoretical model connecting the seniority system with the reelection goals of the legislators: we formulate a full-equilibrium, dynamic model of policy formation in a representative system in which a seniority system emerges endogenously.[2] We develop a model in which both voters and legislators are acting rationally both on and off the equilibrium path. Voters take into account the fact that their representative is only a member of a legislative body and legislators realize that their actions will affect voters' behavior in subsequent elections. All agents take into account the dynamic effects of all of their actions.

Our approach is to model the representative process as an $\mathcal{L} + n$–player stochastic game, where $\mathcal{L}$ is the number of legislators, and n is the number of voters, partitioned into $\mathcal{L}$ distinct districts. The game alternates back and forth between the voter game and the legislative game. The voter game will consist of a game in which all the voters in each of the $\mathcal{L}$ legislative districts vote to determine who will be their representative for the next legislative session. The legislative game will consist of a game in which the legislators decide whether or not to have a seniority system for the current session and then proceed to select a policy. The policy selected is a decision on a distribution of a fixed amount of money among the legislative districts. We shall model the legislative game using the approach of Baron and Ferejohn (1989), who consider the legislative game as a form of a Rubinstein bargaining game: a random recognition rule, which depends on seniority, determines the legislator who makes a proposal. The legislators then vote, by majority rule, whether to accept or reject the proposal. The process continues until the legislature accepts a proposal—at which time the legislature adjourns, and new elections are held (i.e., we return to the voter game).

We show that an equilibrium exists in which the legislature always votes to impose on itself a nontrivial seniority system. In the proposal stage, the proposer selects a minimum winning coalition, retaining $(\mathscr{L} + 1)/2\mathscr{L}$ for its own district and allocating $1/\mathscr{L}$ to the districts of the remaining coalition members. Districts that are not part of the winning coalition get nothing. This proposal passes and the game proceeds to the voter game. Voters always reelect incumbents. The intuition behind the results is that voters, understanding the incentives in the legislative game, realize that their representative will be disadvantaged without seniority.

These results contrast with those found in most formal models of voting. Most formal voting models predict tied elections, with no incumbency effects. In our model the incumbent always wins by a unanimous margin. In addition, we have an endogenously chosen seniority system. These two phenomena are related to each other in that the seniority system and the incumbency effect support each other in equilibrium.

It is tempting to interpret the equilibrium of this model as a situation in which legislators blackmail voters to reelect them through the imposition of the seniority system. However, note that that is not exactly what happens in the model. In our model, the legislators cannot commit future legislatures to adopt a seniority system. The future legislature is free to vote against the seniority system if it is not in the interest of the legislators in that legislature to do so. What drives the incumbency effect in our model is the recognition by voters that self-interested legislators with seniority will vote for a seniority system. If a sufficient number of the other legislators have seniority, then it is in the self-interest of a district to make sure that its legislator does also, since the legislature will undoubtedly impose a seniority system. If all voters think this, it becomes a self-fulfilling prophecy.

THE GENERAL FRAMEWORK

Before introducing the model, we develop some general notation for stochastic games. Our model will be a special case of such a general model. Assume that there is a set N of *players*, a set X of *alternatives*, and for each player $i \in N$, a Von Neumann Morgenstern *utility function* u_i: $X \to \mathbb{R}$ over the set of alternatives. We assume that X contains a *null outcome*, x_0 with $u_i(x_0) = 0$ for all $i \in N$. Let $\mathscr{T}$ be a set of *states*. We now define a *stochastic game*, $\Gamma = \{\Gamma^t: t \in \mathscr{T}\}$ to be a collection of *game elements* $\Gamma^t = (S^t, \pi^t, \psi^t)$. Here $S^t = \Pi_{i \in N} S_i^t$ is an n-tuple of *pure strategy sets*. Next π^t: $S^t \to \mathcal{M}(\mathscr{T}) = \Delta^{|\mathscr{T}|}$ is a *transition function* specifying for each $s^t \in S^t$ a probability distribution, $\pi^t(s^t)$ on $\mathscr{T}$, which determines for each $s^t \in S^t$ and $y \in \mathscr{T}$, the probability $\pi^t(s^t)(y)$ of proceeding to game element Γ^y. Finally, ψ^t: $S^t \to X$ is an *outcome function* that specifies for each $s^t \in S^t$ an outcome $\psi^t(s^t) \in X$. We let $S = \Pi_{t \in \mathscr{T}} S^t$ be the collection of pure strategy n-tuples, one for each game element. We write $\Sigma_i^t = \mathcal{M}(S_i^t)$, where $\mathcal{M}(S_i^t)$ is the set of probability distributions over S_i^t, and then define $\Sigma_i = \Pi_{t \in \mathscr{T}} \Sigma_i^t$ to be the set of *stationary strategies* for player i. Elements of Σ are written in the form $\sigma = (\sigma_1, \sigma_2, \ldots, \sigma_n)$. We also use the abusive notation $\sigma^t(s^t) = \Pi_{i \in N} \sigma_i^t(s_i^t)$ and $\sigma(s) = \Pi_{t \in \mathscr{T}} \sigma^t(s^t)$ to represent the probability under σ of choosing the pure strategy profile $s^t \in S^t$ and $s \in S$, respectively.

For stationary strategies, we can define the payoff function M^t: $\Sigma \to \mathbb{R}^n$ by

$$M_i^t(\sigma) = \sum_{\tau=1}^{\infty} \sum_{r \in \mathscr{T}} \pi_\tau^t(\sigma)(r) \cdot u_i(\psi^r(\sigma^r)), \qquad \textbf{(1)}$$

where $\pi_\tau^t(\sigma)(r)$ is defined inductively by

$$\pi_1^t(\sigma)(r) = \pi^t(\sigma^t)(r) = \sum_{s^t \in S^t} \sigma^t(s^t) \cdot \pi^t(s^t)(r)$$

$$\pi_\tau^y(\sigma)(r) = \sum_{y \in Y} \pi_{\tau-1}^y(\sigma)(y) \cdot \pi_1^y(\sigma^t)(r)$$

and $u_i(\psi^t(\sigma^t))$ is defined by

$$u_i(\psi^t(\sigma^t)) = \sum_{s^t \in S^t} \sigma^t(s^t) \cdot u_i(\psi^t(s^t)).$$

Note that this is only well defined if the sum in equation 1 converges for all σ, t, and i.

A strategy n-tuple, $\sigma \in \Sigma$ is said to be a *Nash equilibrium* if $M_i(\sigma_i', \sigma_{-i}) \leq M_i(\sigma)$ for all $\sigma_i' \in \Sigma_i$. Applying Bellman's optimality principle (see, e.g., Sobel 1971, theorem 3), it follows that any stationary Nash equilibrium can be characterized by a collection $\{v^t\}_{t \in \mathscr{T}} \subseteq \mathbb{R}^n$ of values for each game element Γ^t and a strategy profile, $\sigma \in \Sigma$ satisfying two properties.

P1. *For all $t \in \mathscr{T}$, σ^t is a Nash equilibrium to the game with payoff function G^t: $\Sigma^t \to \mathbb{R}^n$ defined by*

$$G^t(\sigma^t) = u(\psi^t(\sigma^t)) + \sum_{y \in \mathscr{T}} \pi^t(\sigma^t)(y) \cdot v^y$$

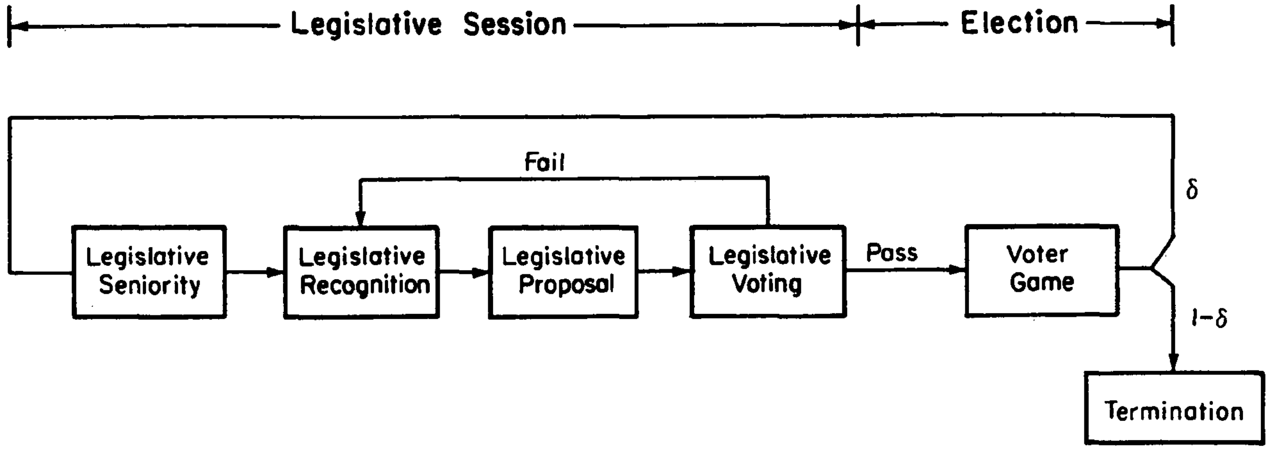
$$= E_{\sigma^t}\left[u(\psi^t(s^t)) + \sum_{y \in \mathscr{T}} \pi^t(s^t)(y) \cdot v^y\right]$$

$$= \textstyle\sum_{s^t \in S^t} \sigma^t(s^t) \cdot \left[u(\psi^t(s^t)) + \displaystyle\sum_{y \in \mathscr{T}} \pi^t(s^t)(y) \cdot v^y\right].$$

P2. *For all $t \in \mathscr{T}$, $v^t = G^t(\sigma^t)$.*

Figure 1

Schematic Diagram of the Seniority Game



We will use this result to characterize equilibria in the stochastic game we consider. Finally, it also follows from Sobel's (1971) results that a Nash equilibrium in the set of stationary strategies is also a Nash equilibrium in the larger class of nonstationary strategies.

THE GAME

We consider an infinitely repeated game between legislators and their constituents.[3] The game alternates back and forth between a legislative session and an election, as illustrated in Figure 1. In the legislative session, the actors are the legislators (one from each legislative district), and the job of the legislature is to divide a dollar between the legislative districts. The legislative session consists of four parts: a vote on the seniority structure, random recognition of a member, a proposal by the randomly selected member, and a vote on the proposal. In the vote on the seniority structure, if a majority of the legislators vote for a seniority system, a seniority system is imposed; otherwise, there is no seniority system. Next, a random recognition rule, like that of Baron and Ferejohn (1989) is used to select a legislator as a proposer. If no seniority system was passed, all legislators have equal probability of being selected. On the other hand, if a seniority system was passed, then the probability of recognition is an increasing function of i's relative seniority. The proposer then proposes a division of the dollar by legislative district. Finally, the legislature votes on the proposal. If the proposal is defeated, a new proposer is selected and the game continues as before, except that in the second round and thereafter seniority is ignored in selecting the proposer.[4] Once a proposal passes the legislature, the legislative session ends, and the game moves to the election.

In the election, the actors are the voters in the legislative districts. In each district, the voters can choose to reelect their incumbent legislator (in which case the legislator has seniority in the next session and receives a salary c) or not to reelect the incumbent (in which case their legislator receives no salary and goes to the next session with no seniority).[5] After each election the legislative game begins again with the new seniority structure.

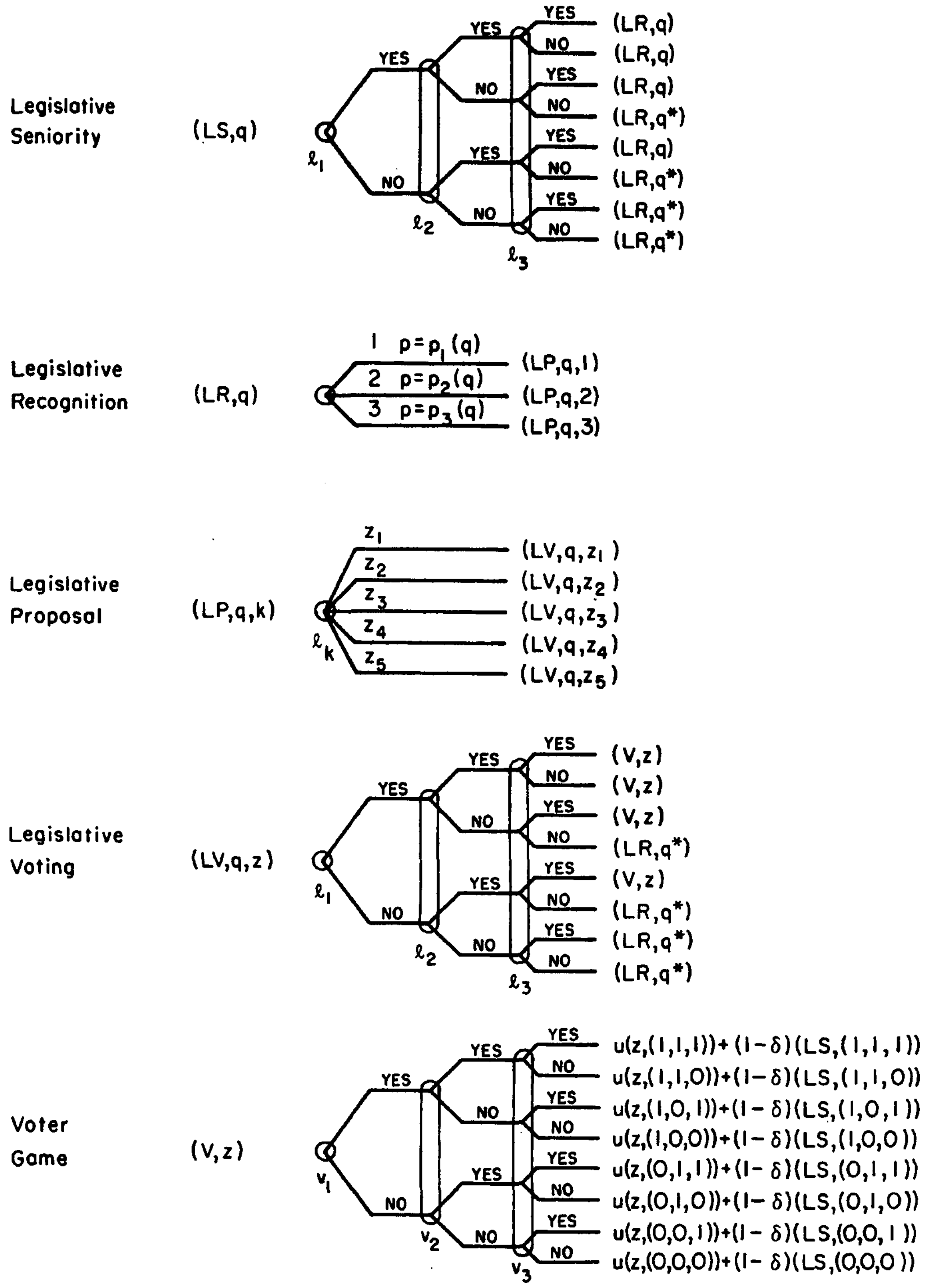
All agents have utility functions that are the discounted present value of their lifetime stream of utility. For the legislators, in each period, payoffs consist of a salary, which depends on whether they are reelected, and a percentage $(1 - \theta)$ of what they secure for their district. Thus, they skim some exogenously given portion of their district's payoff. In each period, the voters get θ times their share of what their legislator is able to secure for the district.

Figure 1 gives a schematic diagram that illustrates the basic components and the sequence of events for the seniority game. We now define the legislative seniority game more formally as a special kind of stochastic game. Each of the aforementioned components will be a game element in the stochastic game. We will specify the strategy sets, transition functions, and outcome functions for each of these game elements. The reader may find it useful to refer to Figure 2 in understanding the following definitions. This figure illustrates the extensive form game tree for each of the game elements in the case in which there are just three legislative districts, one voter per district, and five possible policy choices.

We let $N = L \cup V$, where L is the set of *legislators*, with $\mathcal{L} = |L| \geq 3$ odd, and V is the set of *voters*. We assume that $X' = \Delta^{\mathcal{L}} \times \{0, 1\}^{\mathcal{L}}$, and $X = X' \cup \{x_0\}$. Elements of X' are written in the form $x = (z, q)$, where $z = (z_1, \ldots, z_{\mathcal{L}}) \in Z = \Delta^{\mathcal{L}}$ and $q = (q_1, \ldots, q_{\mathcal{L}}) \in Q = \{0, 1\}^{\mathcal{L}}$. We assume that there is a function $\phi: V \rightarrow L$ identifying the legislative districts, such that voter v is in legislator ℓ's district if $\phi(v) = \ell$. We assume that $n_\ell = |\phi^{-1}(\ell)|$ is odd for all $\ell \in L$. We assume that utility functions over X' are of the form $u_i(x) = (1 - \theta)z_i + cq_i$ for $i \in L$, and $u_i(x) = (\theta/n_{\phi(i)})z_{\phi(i)}$ for $i \in V$. So $Q = \{0, 1\}^{\mathcal{L}}$ represents the seniority

FIGURE 2

Extensive Forms for Each Game Element with Three Legislative Districts, and One Voter per District


Legislative Seniority
(LS,q)
ℓ_1
ℓ_2
ℓ_3
YES
NO
(LR,q)
(LR,q)
(LR,q)
(LR,q*)
(LR,q)
(LR,q*)
(LR,q*)
(LR,q*)
Legislative Recognition
(LR,q)
1 p=p_1(q)
2 p=p_2(q)
3 p=p_3(q)
(LP,q,1)
(LP,q,2)
(LP,q,3)
Legislative Proposal
(LP,q,k)
ℓ_k
z_1
z_2
z_3
z_4
z_5
(LV,q,z_1)
(LV,q,z_2)
(LV,q,z_3)
(LV,q,z_4)
(LV,q,z_5)
Legislative Voting
(LV,q,z)
ℓ_1
ℓ_2
ℓ_3
(V,z)
(V,z)
(V,z)
(LR,q*)
(V,z)
(LR,q*)
(LR,q*)
(LR,q*)
Voter Game
(V,z)
v_1
v_2
v_3
u(z,(1,1,1))+(1−δ)(LS,(1,1,1))
u(z,(1,1,0))+(1−δ)(LS,(1,1,0))
u(z,(1,0,1))+(1−δ)(LS,(1,0,1))
u(z,(1,0,0))+(1−δ)(LS,(1,0,0))
u(z,(0,1,1))+(1−δ)(LS,(0,1,1))
u(z,(0,1,0))+(1−δ)(LS,(0,1,0))
u(z,(0,0,1))+(1−δ)(LS,(0,0,1))
u(z,(0,0,0))+(1−δ)(LS,(0,0,0))

structure of the legislature, with typical element $q = (q_1, \ldots, q_{\mathcal{L}})$. Thus, $q_i = 1$ indicates that legislator i has seniority, whereas $q_i = 0$ indicates it does not have seniority.

Let $0 < \delta < 1$ be a fixed discount rate and q^* be the element of Q satisfying $q_i^* = 1$ for all i. Let $p\colon Q \to \Delta^{\mathcal{L}}$ be a function which indicates the proposal power of each legislator as a function of its seniority (if a seniority system is adopted). We assume that p is strictly monotonic in each component: for all $q \in Q$ and $i \in L$, $q_i > q_i' \Rightarrow p_i(q) > p_i(q_i', q_{-i})$ and that $q_i = q_j \Rightarrow p_i(q) = p_j(q)$. Thus, more seniority means a higher probability that a legislator will be selected as the proposer; and legislators with the same seniority have equal probability of being selected.

There are a total of six game elements, referred to as Termination Game, Legislative Seniority Game, Legislative Recognition Game, Legislative Proposal Game, Legislative Voting Game, and Voter Game. These games are indexed by the corresponding element in the set $\{T, LS, LR, LP, LV, V\}$. Each game element can be further indexed on a state variable. The state spaces for these games are $\emptyset$, Q, Q, $Q \times L$, $Q \times Z$, and Z, respectively. In order to be able to distinguish the difference between the games by means of the state variable we append the identifying element as the first element of the state for each game element. Thus, $\mathcal{T} = (\{T\}) \cup (\{LS\} \times Q) \cup (\{LR\} \times Q) \cup (\{LP\} \times Q \times L) \cup (\{LV\} \times Q \times Z) \cup (\{V\} \times Z)$ is the set of possible states.[6] We now define the strategy sets and transition functions for the game elements.

Termination Game

For $t = T \in \{T\}$,

$$S_i^t = \{0\} \quad \text{if} \quad i \in N.$$

For all $s^t \in S^t$,

$$\pi^t(s^t)(T) = 1$$

and

$$\psi^t(s^t) = x_0.$$

The Termination Game is just a dummy game element that occurs when the game ends. It is indicated by $t = T$. After each election, the stochastic game proceeds to the termination game with probability $1 - \delta$. In the termination game, each agent has a dummy strategy set, consisting of the singelton $\{0\}$; and the transition function π^t specifies that for any strategy choice, we remain in Termination Game with probability 1. In each period, the null outcome occurs, giving all agents utility 0. Hence, once the Termination Game is reached, all agents get 0 ever after. This is a formal way of introducing discounting into the model. In expected value, this makes utility in other parts of the game equal to the discounted current value of the future utility stream, where players discount future payoffs by a factor δ.

Legislative Seniority Game

For $t = (t_0, t_1) \in \{LS\} \times Q$,

$$S_i^t = \begin{cases} \{0, 1\} & \text{if } i \in L \\ \{0\} & \text{if } i \in N - L. \end{cases}$$

For all $s^t \in S^t$,

$$\pi^t(s^t)(LR, t_1) = 1 \quad \text{if} \quad \Sigma_{i \in L}\, s_i^t > \frac{\mathcal{L}}{2},$$

$$\pi^t(s^t)(LR, q^*) = 1 \quad \text{if} \quad \Sigma_{i \in L}\, s_i^t \leq \frac{\mathcal{L}}{2},$$

and

$$\psi^t(s^t) = x_0.$$

The Legislative Seniority Game is the first stage of the legislative session. In this game, the legislature votes whether or not to have seniority for the current session. This game is indexed by $t = (S, t_1)$, where $t_0 = S$ indicates that we are in the Legislative Seniority Game, and t_1 is the current seniority vector. Since this game is a game between legislators, only the legislators have nontrivial strategy sets in the game. The strategy set for a legislator i is the set $\{0, 1\}$, where 0 represents a *no* vote, and 1 represents a *yes* vote. The transition function π^t indicates that we proceed with probability 1 to the Legislative Recognition Game. The vote determines which seniority vector is used in the Legislative Recognition Game. If a majority of the legislators vote for seniority, then the current seniority vector, t_1, is used in the Legislative Recognition Game. If there is not a strict majority for seniority, then the seniority vector q^*, which assigns equal weight to all legislators, is used.

Legislative Recognition Game

For $t = (t_0, t_1) \in \{LR\} \times Q$,

$$S_i^t = \{0\} \quad \text{if} \quad i \in N.$$

For all $s^t \in S^t$ and $y \in L$,

$$\pi^t(s^t)(LP, t_1, y) = p_y(t_1)$$

and

$$\psi^t(s^t) = x_0.$$

The Legislative Recognition Game is the second stage of the legislative session. This game is indexed by $t = (LR, t_1)$, where $t_0 = LR$ indicates we are in the Legislative Recognition Game, and t_1 is the current seniority vector. All agents have singleton strategy sets in this game. Thus, this game is just a chance move, which selects the legislator to make a proposal. From this game, we go with probability 1 to the Legislative Proposal Game. The probability that any legislator is selected is determined by the function $p_y(t_1)$. If seniority passed in the Legislative Seniority Game, the argument of p_y is t_1, which is the vector of seniorities of the legislators. If seniority failed, then $t_1 = q^*$ is used for the seniority vector. A legislator is selected by a random recognition rule to make a proposal for consideration by the legislature. This rule is similar to the Baron–Ferejohn recognition rule, except that we let the recognition rule be a function of seniority. Assumptions made above about p_y guarantee that higher seniority leads to higher probability of being selected.

Legislative Proposal Game

For $t = (t_0, t_1, t_2) \in \{LP\} \times Q \times L$,

$$S_i^t = \begin{cases} Z & \text{if } i = t \\ \{0\} & \text{if } i \in N - \{t\}. \end{cases}$$

For all $s^t \in S^t$,

$$\pi^t(s^t)(LV, t_1, s_t^t) = 1$$

and

$$\psi^t(s^t) = x_0.$$

The Legislative Proposal Game is the third stage of the legislative session. This game is indexed by $t = (LP, t_1, t_2)$, where $t_0 = LP$ indicates that we are in the Legislative Proposal Game; $t_1 \in Q$ is the current seniority structure that has been approved by the legislature; and $t_2 \in L$ represents the legislator who has been selected to make a proposal. In this game element, the only legislator with a nontrivial strategy set is legislator $j = t_2$, whose strategy set is Z, the set of possible divisions of the dollar between the legislative districts. If the legislator proposes the division z, then we proceed with probability 1 to the Legislative Voting Game (LV, t_1, z).

Legislative Voting Game

For $t = (t_0, t_1, t_2) \in \{LV\} \times Q \times Z$,

$$S_i^t = \begin{cases} \{0, 1\} & \text{if } i \in L \\ \{0\} & \text{if } i \in V. \end{cases}$$

For all $s^t \in S^t$,

$$\pi^t(s^t)(V, t_1) = 1 \quad \text{if} \quad \Sigma_{i \in L}\, s_i^t > \frac{\mathcal{L}}{2},$$

$$\pi^t(s^t)(LP, q^*) = 1 \quad \text{if} \quad \Sigma_{i \in L}\, s_i^t \leq \frac{\mathcal{L}}{2},$$

and

$$\psi^t(s^t) = x_0.$$

The Legislative Voting Game is the fourth and last stage of the legislative session. This game is indexed by $t = (LV, t_1, t_2)$, where $t_0 = LV$ indicates that we are in the Legislative Voting Game, $t_1 \in Q$ indicates the current seniority vector, and $t_2 \in Z$ indicates the proposal for division of the dollar that was selected by the proposer in the Legislative Proposal Game. In this game, each legislator has the strategy set $\{0, 1\}$. Here, a zero represents a *no* vote, a one a *yes* vote, on the proposal t_1. If the legislators vote to accept the proposal, the legislative session ends, and we proceed with probability 1 to the Voter Game. If the legislators reject the proposal, then we return with probability 1 to the Legislative Recognition Game, with the exception that seniority is ignored in selecting the proposer. We shall discuss what happens if seniority is used in subsequent proposal rounds. Note that the Legislative Proposal and Legislative Voting Games together are similar to the closed rule version of the Baron–Ferejohn model.

Voter Game

For $t = (t_0, t_1) \in \{V\} \times Z$,

$$S_i^t = \begin{cases} \{0, 1\} & \text{if } i \in V \\ \{0\} & \text{if } i \in L. \end{cases}$$

For all $s^t \in S^t$,

$$\pi^t(s^t)(y) = \begin{cases} \delta & \text{if } y = (S, q(s^t)) \\ 1 - \delta & \text{if } y = T \end{cases}$$

and

$$\psi^t(s^t) = (t_1, q(s^t)),$$

where $q(s^t) = (q_1(s^t), q_2(s^t), \ldots, q_{\mathcal{L}}(s^t)) \in Q$ is defined by

$$q_i(s^t) = \begin{cases} 1 & \text{if } \sum_{j \in \phi^{-1}(i)} s_j^t > \dfrac{n_\ell}{2} \\ 0 & \text{if } \sum_{j \in \phi^{-1}(i)} s_j^t \leq \dfrac{n_\ell}{2} \end{cases}$$

and where $0 < \theta < 1$ and $0 < c$ are constants. The Voter Game consists of a set of simultaneous elections in all of the legislative districts. This game is indexed by $t = (V, t_1)$, where $t_0 = V$ indicates that we are in the Voter Game, and $t_1 \in Z$ represents the outcome of the Legislative Voting Game. In each legislative district, the voters of that district vote whether or not to reelect their legislator. In the version of the game as it is presented here, there is only one legislator in each district and no challenger. So the effect of a negative vote in a given district is that the legislator from that district does not get a salary for the next period and loses seniority.

This completes the description of the stochastic game. Note that there are no payoffs except in the voter game. At that point policy $x = (t_1, q(s^t))$ is implemented. Thus, the pie is divided up among the districts according to $z = t_1 \in \Delta^{\mathcal{L}}$, and $q(s^t) \in Q$ determines which legislators get reelected and which do not. Given the utility functions we have specified, it follows that the output $t_{1\ell}$ to district ℓ is first divided up with $\theta t_{1\ell}$ actually delivered to the voters and $(1 - \theta)t_{1\ell}$ being skimmed off by legislator ℓ. The voters each get an even share of the delivered output. The legislators, in addition to their share of the output, get a salary that is dependent on whether they are reelected or not.

Figure 2 presents a schematic depiction of the game for the case of three legislators, three voters (one per district), and five possible proposals. The Legislative Seniority vote determines the probabilities in the Legislative Recognition Game. A proposer is selected who makes a proposal in the Legislative Proposal Game. The legislature votes on this proposal. If it passes, the game proceeds to the Voter Game and a new election is held leading to a new session of the legislature. (The discounting comes in here.) If the proposal fails, the game goes back to the Legislative Recognition Game, with seniority not used to determine the probabilities of recognition. From here, the game proceeds as before.

RESULTS

Our main result is that there exists an equilibrium to the stochastic game in which a seniority system is adopted and incumbent legislators always get reelected.

PROPOSITION 1. *The following is a stationary equilibrium to the stochastic game defined in the previous section:*

1. *Legislative Seniority Game. For $t \in \{LS\} \times Q$, and $i \in L$,*

$$\sigma_i^t(t_{1i}) = 1$$

2. *Legislative Proposal Game. For $t \in \{LP\} \times Q \times L$,*

$$\sigma_t^t = \frac{1}{|\Omega_t|} \sum\nolimits_{w \in \Omega_t} \delta_{z_t(w)},$$

where $\Omega_t = \{\omega \in \{0, 1\}^{\mathcal{L}}: \Sigma_i \omega_i = (\mathcal{L} + 1)/2,\ \omega_t = 1\}$, δ_x is the Dirac delta at x, and $z_t: \Omega_t \to \mathbb{R}^{\mathcal{L}}$ is defined by

$$z_{ti}(\omega) = \begin{cases} \dfrac{\mathcal{L}+1}{2\mathcal{L}} & \text{if } i = t \\[2ex] \dfrac{1}{\mathcal{L}} & \text{if } i \neq t,\ \omega_i = 1 \\[2ex] 0 & \text{otherwise.} \end{cases}$$

3. *Legislative Voting Game. For $t \in \{LV\} \times Q \times Z$, and $i \in L$,*

$$\sigma_i^t(1) = \begin{cases} 1 & \text{if } t_{1i} \geq \dfrac{1}{\mathcal{L}} \\[2ex] 0 & \text{if } t_{1i} < \dfrac{1}{\mathcal{L}}. \end{cases}$$

4. *Voter Game. For $t \in \{V\} \times Z$, and $i \in V$,*

$$\sigma_i^t(1) = 1.$$

Proof. The formal proof is in the Appendix. The proof is straightforward, and simply consists of verifying that these strategies satisfy the Bellman conditions that characterize a Nash equilibrium of a stochastic game. These conditions are given as conditions P1 and P2. To verify this, we first specify values, v^t, for each game element. These values represent the value of the game element if equilibrium behavior is followed forever after. The Bellman conditions require that if we replace all game elements by their values, then for each game element, fixing the values of the other game elements, the specified strategies are a Nash equilibrium for that game element (P1), and the computed value of each game element at the equilibrium strategies is equal to the specified value (P2). While the proof is straightforward, it is tedious, since each of these conditions must be verified for all of the six game elements.

Remarks. The proposition gives equilibrium strategies for both the legislators and voters in the stochastic game. We first describe the equilibrium and then provide some intuition for the result. In the Legislative Seniority Game all legislators who have seniority vote in favor of the seniority system, and those who do not have seniority vote against the seniority system. Since in equilibrium all legislators get reelected the, seniority system always passes.

In the Legislative Proposal game, the proposer selects a minimal winning coalition of legislators, which includes itself. The proposer retains $(\mathcal{L} + 1)/2\mathcal{L}$ for its own district, leaving $1/\mathcal{L}$ to be allocated to the districts of each of the remaining members of the coalition. Districts that are not a part of the winning coalition are allocated 0. Thus, the proposer obtains a premium of $(\mathcal{L} + 1)/2\mathcal{L} - 1/\mathcal{L} = (\mathcal{L} - 1)/2\mathcal{L}$ due to its proposal power. As $\mathcal{L} \to \infty$, the premium increases with a limit of one-half.

In the Legislative Voting Game, a legislator votes for a proposal if and only if the legislator receives at least $1/\mathcal{L}$. Thus, if the proposer has proposed an equilibrium proposal, it will pass. Finally, in the Voter Game, the voters always vote to reelect their legislators. It should be noted that although the proof shows only that this is a Nash equilibrium for the voters, in fact, the strategy of voting for the incumbent is a dominant strategy for the voters in any given legislative district, taking the behavior in the remainder of the game as fixed.

Several features of this equilibrium stand in sharp contrast to the results that come out of the traditional voting literature. Most voting models predict tied elections, with no incumbency effects. Here, we obtain instead equilibrium behavior by the voters in which the incumbent wins by a large (unanimous) margin. The intuition behind the result is simple: the voters know that in equilibrium the seniority system will pass and thus that it is in the their best interest to reelect the incumbent, since a senior legislator will be more easily able to serve the constituency than a junior legislator. Note that voters do not know that there will be a seniority system in the next session but, rather, know that in the steady-state equilibrium, seniority will be voted in each session.

Finally, we emphasize that we have only identified one equilibrium of this model. There may be other equilibria. In fact, we believe that there would also be an equilibrium in which seniority is rejected by the legislature and all legislators are defeated for reelection. This later equilibrium may be able to be refined away by appropriate equilibrium refinements, since voters would all be indifferent, in equilibrium, between voting for the incumbent and voting for the challenger.

EXTENSIONS

The Power of the Initial Proposal

In our model the seniority system works through the proposal stage, by influencing the probability that legislators will get chosen to be the proposer. We assumed that seniority is only used for selecting the proposer in the first round of any legislative session. Thus, if a proposal is turned down in the legislature, seniority is no longer used to select the proposer during that legislative session. In a companion paper we analyze the case in which seniority is in effect throughout the legislative session (McKelvey and Riezman (n.d.). One might think that this system (which, on its face, gives more power to the senior members) would make them better off and hence would be selected by them. However, we show that the opposite is the case. When seniority is in effect for the entire session, there is no symmetric stationary equilibrium in which senior and nonsenior legislators have different values and in which voters always reelect incumbents. In other words, when seniority is used throughout the session, there is no equilibrium in which seniority has benefits for legislators. Thus, legislators would be indifferent between having, and not having, such a seniority system and would hence prefer a seniority system in which seniority is used only for the first proposal in each legislative session. Thus, we get the rather paradoxical result that legislators who have seniority would choose a seniority system that on its face gives less power to senior members.

It is worth pointing out that a seniority system that gives only initial proposal power is a realistic description of the seniority system for the U.S. Congress in the sense that seniority is embodied in the committee system. The committees make proposals by sending bills to the floor. Once the bills go to the floor, the committees lose most of their power, since bills that are amended or defeated generally do not go back to committee in that session. Hence, our model might explain certain features about how seniority systems are set up—in particular, the importance of initial proposal power.

We briefly describe the result from the companion paper. The game is altered by changing the Legislative Voting Game so that when a proposal is rejected, the subsequent Legislative Recognition Game will use the original seniority vector. The rest of the stochastic game is as before. In other words, the Legislative Voting Game is

The Revised Legislative Voting Game

For $t \in \{LV\} \times Z$,

$$S_i^t = \begin{cases} \{0, 1\} & \text{if } i \in L \\ \{0\} & \text{if } i \in V. \end{cases}$$

For all $s^t \in S^t$,

$$\pi^t(s^t)(V, t_1) = 1 \quad \text{if} \quad \Sigma_{i \in L}\, s_i^t > \frac{\mathcal{L}}{2},$$

$$\pi^t(s^t)(LR, t_1) = 1 \quad \text{if} \quad \Sigma_{i \in L}\, s_i^t \leq \frac{\mathcal{L}}{2},$$

and

$$\psi^t(s^t) = x_0.$$

Consider the stochastic game of the previous section, substituting the above game for the previous Legislative Voting Game. Call this the Revised Game. The following proposition is proved in McKelvey and Riezman (n.d.).

PROPOSITION 2. *In the Revised Game there is no symmetric stationary equilibrium with the following two properties:*

1. *Voters always reelect incumbents:*

   For $t \in \{V\} \times Z$, and $i \in V$: $\sigma_i^t(1) = 1$ for all i.

2. *The value of senior and nonsenior members in the Legislative Seniority Game is different.*

The intuition behind this result has to do with how proposers choose coalition partners. Once chosen, proposers want to include in the coalition those with the lowest continuation values, because they can be given less and will still vote for the proposal. It follows, then, when seniority is used throughout the legislative session, that if seniority benefits senior members, they will be less likely to be included in coalitions. Proposition 2 shows that this effect of being chosen less often swamps any other advantages of using seniority throughout the session. Thus, once the proposer is chosen, senior members want to look like nonsenior members so they are as likely to be included in the coalition.

Ordinal Seniority

Our model assumes a binary seniority system: a legislator either has seniority or does not. The next step is to introduce the possibility of ordinal seniority, where a legislator's seniority measures the number of times the legislator has been reelected. The congressional seniority system is an ordinal system.

If each legislator has an independent probability of dying or retiring, then at any point in time, the distribution of seniority, even in equilibrium, could be heterogeneous. Depending on the distribution of seniority and exactly how seniority affects the probability of being selected as the proposer, a seniority system would not necessarily be adopted by the legislature in the first place. Hence, with ordinal seniority, it appears that seniority system may occasionally be defeated. We plan to explore this issue in future work.

CONCLUSION

We have developed a formal model of voter behavior and legislative decision making in which the seniority system and the incumbency effects emerge as an equilibrium. There are a number of weaknesses in this model. We have assumed an unrealistically simple model of the legislative session and of how seniority plays a role; that is, the legislative session is characterized by a random recognition voting game similar to the Baron–Ferejohn model. Second, we assume that the only decision made by the legislature is a decision on the division of a fixed pie. We also assume that legislators' preferences are a function of how much they get for their constituents, rather than just whether they are reelected. We hope to remedy some of these weaknesses in the future. Despite these obvious weaknesses of the model, the model illustrates that it is possible to construct consistent formal models that connect legislative organization with reelectoral goals of legislators. Within the context of this model we have shown that when the effect of seniority is to change the probability of recognition on the first round, seniority has value. If seniority changes the probability of recognition on every round, then senior members get no benefits from seniority.

APPENDIX: PROOF OF PROPOSITION 1

We first specify the values, v^t, associated with these strategies. We then verify that for these values, property P1 and P2 are satisfied. For the following equations, we set $w_1 = (\mathscr{L} + 1)/2\mathscr{L}$, and $w_2 = 1/\mathscr{L}$. Also, we define $Z^1 = \{z \in Z: |\{j \in L: z_j \geq w_2\}| > \mathscr{L}/2\}$, and $Z^0 = Z - Z^1$. Similarly, define $Q^1 = \{q \in Q: |\{i \in L: q_i = 1\}| > \mathscr{L}/2\}$, and $Q^0 = Q - Q^1$.

The values of the games are defined below. To interpret these values, go to the definitions of the individual games. For example, for $t \in \{LS\} \times Q$, you are in the Legislative Seniority Game. $v_i^t = v_i^{(R,\, t_1)}$ means that the value of the seniority game given that seniority has passed ($t_1 \in Q^1$) is the value in the Legislative Recognition Game with seniority vector t_1. If seniority does not pass ($t_1 \in Q^0$), then the value of the game is given by the value in the Legislative Recognition Game with seniority vector q^*. Other values are defined in a similar way.

For $t \in \{T\}$,

$$v_i^t = 0 \text{ for all } i \in N.$$

For $t \in \{LS\} \times Q$,

$$v_i^t = v_i^{(R,\, t_1)} \text{ if } t_1 \in Q^1,$$

$$v_i^t = v_i^{(R,\, q^*)} \text{ if } t_1 \in Q^0.$$

For $t \in \{LR\} \times Q$,

$$v_i^t = (1 - \theta)\left[p_i(t_1)w_1 + \frac{1}{2}\sum_{y \in \mathscr{L} - \{i\}} p_y(t_1)w_2\right] + c + \delta v_i^* \quad \text{if } i \in L,$$

$$v_i^t = \frac{\theta}{n_{\phi(i)}}\left[p_{\phi(i)}(t_1)w_1 + \frac{1}{2}\sum_{y \in \mathscr{L} - \{\phi(i)\}} p_y(t_1)w_2\right] + \delta v_i^* \quad \text{if } i \in V,$$

where

$$v_i^* = v_i^{(R,\, q^*)} = \begin{cases} \dfrac{1}{1-\delta}\left[\dfrac{1}{\mathscr{L}}(1-\theta) + c\right] & \text{if } i \in L, \\[2ex] \dfrac{1}{1-\delta}\left[\dfrac{\theta}{\mathscr{L}n_{\phi(i)}}\right] & \text{if } i \in V. \end{cases}$$

For $t \in \{LP\} \times Q \times L$,

$$v_i^t = (1 - \theta)w_1 + c + \delta v_i^* \qquad \text{if } i = t,$$

$$v_i^t = \frac{1}{2}(1 - \theta)w_2 + c + \delta v_i^* \qquad \text{if } i \neq t,$$

$$v_i^t = \frac{\theta}{n_{\phi(i)}} w_1 + \delta v_i^* \qquad \text{if } \phi(i) = t,$$

$$v_i^t = \frac{1}{2}\frac{\theta}{n_{\phi(i)}} w_2 + \delta v_i^* \qquad \text{if } \phi(i) \neq t.$$

For $t \in \{LV\} \times Q \times Z^1$,

$$v_i^t = (1 - \theta)t_{1i} + c + \delta v_i^* \qquad \text{if } i \in L,$$

$$v_i^t = \theta t_{1\phi(i)} + \delta v_i^* \qquad \text{if } i \in V.$$

For $t \in \{LV\} \times Q \times Z^0$,

$$v_i^t = v_i^* \qquad \text{if } i \in N.$$

For $t \in \{V\} \times Z$,

$$v_i^t = (1 - \theta)t_{1i} + c + \delta v_i^* \qquad \text{if } i \in L,$$

$$v_i^t = \frac{\theta}{n_{\phi(i)}} t_{1\phi(i)} + \delta v_i^* \qquad \text{if } i \in V.$$

The next step in the proof is to verify property P2, which requires that for each game and each player the payoffs correspond to the values we have specified. To do this, we start with the definition of G and

then, using the definitions of the game elements and the equilibrium strategies, show that the payoffs equal the appropriate values.
For $t \in \{T\}$,

$$G^t(\sigma^t) = E_{\sigma^t}\left[u(\psi^t(s^t)) + \sum_{y \in \mathcal{T}} \pi^t(s^t)(y)v^y\right]$$

$$= u(x_0) + \pi^t(\sigma^t)(t)v^t = v^t.$$

For $t \in \{LS\} \times Q$,

$$G_i^t(\sigma^t) = E_{\sigma^t}\left[u_i(\psi^t(s^t)) + \sum_{y \in \mathcal{T}} \pi^t(s^t)(y)v_i^y\right]$$

$$= u_i(\psi^t(t_1)) + \sum_{y \in \mathcal{T}} \pi^t(t_1)(y)v_i^y$$

$$= \begin{cases} v_i^{(LR,\, t_1)} & \text{if} \quad \sum_{i \in L} t_{1i} > \dfrac{\mathcal{L}}{2} \\ v_i^{(LR,\, q^*)} & \text{if} \quad \sum_{i \in L} t_{1i} \leq \dfrac{\mathcal{L}}{2} \end{cases}$$

$$= \begin{cases} v_i^{(LR,\, t_1)} & \text{if} \quad |\{i \in L\colon t_{1i} = 1\}| > \dfrac{\mathcal{L}}{2} \\ v_i^{(LR,\, q^*)} & \text{if} \quad |\{i \in L\colon t_{1i} = 1\}| \leq \dfrac{\mathcal{L}}{2} \end{cases}$$

$$= \begin{cases} v_i^{(LR,\, t_1)} & \text{if} \quad t_1 \in Q^1 \\ v_i^{(LR,\, q^*)} & \text{if} \quad t_1 \in Q^0. \end{cases}$$

$$= v^t.$$

For $t \in \{LR\} \times Q$,

$$G^t(\sigma^t) = E_{\sigma^t}\left[u(\psi^t(\sigma^t)) + \sum_{y \in \mathcal{T}} \pi^t(\sigma^t)(y)v^y\right]$$

$$= u(x_0) + \sum_{y \in L} p_y(t_1)v^y.$$

So, for $i \in L$,

$$G_i^t(\sigma^t) = p_i(t_1)v_i^i + \sum_{y \in \mathcal{L} - \{i\}} p_y(t_1)v_i^y$$

$$= p_i(t_1)[(1-\theta)w_1 + c + \delta v_i^*]$$

$$+ \sum_{y \in \mathcal{L} - \{i\}} p_y(t_1)\left[\frac{1}{2}(1-\theta)w_2 + c + \delta v_i^*\right]$$

$$= (1-\theta)\left[p_i(t_1)w_1 + \frac{1}{2}\sum_{y \in \mathcal{L} - \{i\}} p_y(t_1)w_2\right]$$

$$+ c + \delta v_i^* = v_i^t$$

and for $i \in V$,

$$G_i^t(\sigma^t) = p_{\phi(i)}(t_1)v_i^{\phi(i)} + \sum_{y \in \mathcal{L} - \{\phi(i)\}} p_y(t_1)v_i^y$$

$$= p_{\phi(i)}(t_1)\left[\frac{\theta}{n_{\phi(i)}} w_1 + \delta v_i^*\right]$$

$$+ \sum_{y \in \mathcal{L} - \{\phi(i)\}} p_y(t_1)\left[\frac{\theta}{2n_{\phi(i)}} w_2 + \delta v_i^*\right]$$

$$= \frac{\theta}{n_{\phi(i)}}\left[p_{\phi(i)}(t_1)w_1 + \sum_{y \in \mathcal{L} - \{\phi(i)\}} \frac{1}{2} p_y(t_1)w_2\right]$$

$$+ \delta v_i^* = v_i^t.$$

For $t \in \{LP\} \times Q \times L$,

$$G^t(\sigma^t) = E_{\sigma^t}\left[u(\psi^t(s^t)) + \sum_{y \in \mathcal{T}} \pi^t(s^t)(y)v^y\right]$$

$$= u(x_0) + E_{\sigma^t}[v^{(LV,\, s_t^t)}].$$

But since $\sigma_t^t(Z^1) = 1$, we have, for $i \in L$,

$$G^t(\sigma^t) = E_{\sigma^t}[(1-\theta)s_t^t + c + \delta v^*]$$

$$= (1-\theta)E_{\sigma^t}[s_t^t] + c + \delta v^*.$$

But

$$E_{\sigma^t}[s_t^t] = E_{\sigma^t}\left[\frac{1}{|\Omega_t|}\sum_{w \in \Omega_t} \delta_{z_t(w)}\right] = \frac{1}{|\Omega_t|}\sum_{w \in \Omega_t} E_{\sigma^t}[\delta_{z_t(w)}]$$

$$= \frac{1}{|\Omega_t|}\sum_{w \in \Omega_t} z_t(\omega).$$

So

$$E_{\sigma^t}[s_{ti}^t] = \frac{1}{|\Omega_t|}\sum_{w \in \Omega_t} z_{ti}(\omega) = \frac{1}{|\Omega_t|} \cdot |\Omega_t| \frac{\mathcal{L}+1}{2\mathcal{L}}$$

$$= \frac{\mathcal{L}+1}{2\mathcal{L}} = w_1 \text{ if } i = t$$

$$E_{\sigma^t}[s_{ti}^t] = \frac{2\left[\dfrac{\mathcal{L}-1}{2}\right]!}{(\mathcal{L}-1)!} \cdot \frac{(\mathcal{L}-2)!}{2\left[\dfrac{\mathcal{L}-3}{2}\right]!} \cdot \frac{1}{\mathcal{L}}$$

$$= \frac{\left[\dfrac{\mathcal{L}-1}{2}\right]}{(\mathcal{L}-1)} \cdot \frac{1}{\mathcal{L}} = \frac{1}{2\mathcal{L}} = \frac{1}{2}w_2 \quad \text{if} \quad i \neq t.$$

Thus,

$$G_i^t(\sigma^t) = \begin{cases} (1-\theta)w_1 + c + \delta v_i^* & \text{if} \quad i = t \\ \dfrac{1}{2}(1-\theta)w_2 + c + \delta v_i^* & \text{if} \quad i \neq t \end{cases}$$

$$= v_i^t$$

and for $i \in V$,

$$G_i^t(\sigma^t) = E_{\sigma^t}\left[\frac{\theta}{n_{\phi(i)}} s_{ti}^t + \delta v_i^*\right] = \frac{\theta}{n_{\phi(i)}} E_{\sigma^t}[s_{ti}^t] + \delta v_i^*$$

$$= \begin{cases} \dfrac{\theta}{n_{\phi(i)}} w_1 + \delta v_i^* & \text{if} \quad \phi(i) = t \\ \dfrac{1}{2}\dfrac{\theta}{n_{\phi(i)}} w_2 + \delta v_i^* & \text{if} \quad \phi(i) \neq t \end{cases}$$

$$= v_i^t.$$

For $t \in \{LV\} \times Q \times Z^1$, since $t_1 \in Z^1$, it follows that

$$\sigma^t\left(\sum_{i \in L} s_i^t > \frac{\mathscr{L}}{2}\right) = 1.$$

So $\pi^t(\sigma^t)(V, t_1) = 1$. Hence,

$$G_i^t(\sigma^t) = E_{\sigma^t}\left[u_i(\psi^t(s^t)) + \sum_{y \in \mathscr{T}} \pi^t(s^t)(y)v_i^y\right]$$

$$= u(x_0) + \pi^t(\sigma^t)(t_1, V)v_i^{(V, t_1)} + \pi^t(\sigma^t)$$

$$(q^*, 2)v_i^{(LR, q^*)}$$

$$= v_i^{(V, t_1)} = \begin{cases} (1 - \theta)t_{1i} + c + \delta v_i^* & \text{if} \quad i \in L \\ \theta t_{1\phi(i)} + \delta v_i^* & \text{if} \quad i \in V \end{cases}$$

$$= v_i^t.$$

For $t \in \{LV\} \times Q \times Z^0$, since $t_1 \in Z^0$, it follows that

$$\sigma^t\left(\sum_{i \in L} s_i^t \leq \mathscr{L}/2\right) = 1,$$

so $\pi^t(\sigma^t)(q^*, 2) = 1$. Hence,

$$G_i^t(\sigma^t) = E_{\sigma^t}\left[u_i(\psi^t(s^t)) + \sum_{y \in \mathscr{T}} \pi^t(s^t)(y)v_i^y\right]$$

$$= u(x_0) + \pi^t(\sigma^t)(t_1, V)v_i^{(V, t_1)}$$

$$+ \pi^t(\sigma^t)(q^*, 2)v_i^{(LR, q^*)}$$

$$= v_i^{(LR, q^*)} = v_i^* = v_i^t.$$

For $t \in \{V\} \times Z$,

$$\pi^t(s^t)(y) = \begin{cases} \delta & \text{if} \quad y = (S, q(s^t)) \\ 1 - \delta & \text{if} \quad y = T, \end{cases}$$

$$\sigma_i^t(1) = 1 \text{ for all } i.$$

$$G^t(\sigma^t) = E_{\sigma^t}\left[u(\psi^t(s^t)) + \sum_{y \in \mathscr{T}} \pi^t(s^t)(y)v^y\right]$$

$$= E_{\sigma^t}[u(t_1, q(s^t))] + \delta\pi^t(\sigma^t)(q(s^t), 1)v^{(S, q(s^t))}$$

$$= u(t_1, q^*) + \delta v^{(S, q^*)} = u(t_1, q^*) + \delta v^*$$

So, for $i \in L$,

$$G_i^t(\sigma^t) = (1 - \theta)t_{1i} + cq_i^* + \delta v_i^*$$

$$= (1 - \theta)t_{1i} + c + \delta v_i^* = v_i^t$$

and for $i \in V$,

$$G_i^t(\sigma^t) = \frac{\theta}{n_{\phi(i)}} t_{1\phi(i)} + \delta v_i^* = v_i^t.$$

We next verify that property P1 is satisfied, that is, that σ^t is a Nash equilibrium. For each game element we show that no player can benefit from playing a different strategy.

For $t \in \{V\} \times Z$, we want to show that σ^t is a Nash equilibrium to the game with payoff function G^t, where $\sigma_i^t(1) = 1$ for all $i \in V$. It suffices to show that for each $i \in V$, σ_i^t is at least as good as any pure strategy $s_i' \in S_i^t$. So,

$$G_i^t(\sigma^t) \geq G_i^t(\sigma_i', \sigma_{-i}^t)$$

$$\Leftrightarrow E_{\sigma^t}\left[u_i(\psi^t(s^t)) + \sum_{y \in \mathscr{T}} \pi^t(s^t)(y)v_i^y\right]$$

$$\geq E_{\sigma_{-i}^t}\left[u_i(\psi^t(s_i', s_{-i}^t)) + \sum_{y \in \mathscr{T}} \pi^t(s_i', s_{-i}^t)(y)v_i^y\right]$$

for all $s_i' \in S_i^t$. Writing $\underline{1}$ for the $|V|$ component vector of ones, we can rewrite this inequality as

$$u_i(\psi^t(\underline{1})) + \sum_{y \in \mathscr{T}} \pi^t(\underline{1})(y)v_i^y$$

$$\geq u_i(\psi^t(s_i', \underline{1}_{-i})) + \sum_{y \in \mathscr{T}} \pi^t(s_i', \underline{1}_{-i})(y)v_i^y$$

$$\Leftrightarrow \delta v_i^{(S, q(\underline{1}))} \geq \delta v_i^{(S, q(s_i', \underline{1}_{-i}))} \Leftrightarrow v_i^* \geq v_i^{(S, q(s_i', \underline{1}_{-i}))}$$

But $q(s_i', \underline{1}_{-i}) \geq \underline{1} - \varepsilon_\ell$, where $\ell = \phi(i)$, and ε_ℓ is the ℓth standard basis vector. Further, since $\underline{1} - \varepsilon_\ell \in Q^1$, we have $q(s_i', \underline{1}_{-i}) \in Q^1$. So $v_i^{(S, q(s_i', \underline{1}_{-i}))} = v_i^{(LR, q(s_i', \underline{1}_{-i}))}$. Hence, the above inequality can be written

$$v_i^* \geq \frac{\theta}{n_{\phi(i)}}\left[p_{\phi(i)}(q(s_i', \underline{1}_{-i}))w_1\right.$$

$$\left. + \frac{1}{2} \sum_{y \in \mathscr{L} - \{\phi(i)\}} p_y(q(s_i', \underline{1}_{-i}))w_2\right] + \delta v^*{}_i$$

$$\Leftrightarrow \frac{\theta}{\mathscr{L}n_{\phi(i)}} \geq \frac{\theta}{n_{\phi(i)}}\left[p_{\phi(i)}(q(s_i', \underline{1}_{-i}))w_1\right.$$

$$\left. + \frac{1}{2}(1 - p_{\phi(i)}(q(s_i', \underline{1}_{-i})))w_2\right]$$

$$\Leftrightarrow \frac{1}{\mathscr{L}} = p_i(q_0)w_1 + \frac{1}{2}(1 - p_i(q_0))w_2$$

$$\geq p_i(q(s_i', \underline{1}_{-i}))w_1 + \frac{1}{2}(1 - p_i(q(s_i', \underline{1}_{-i})))w_2$$

$$\Leftrightarrow [p_i(q_0) - p_i(q(s_i', s_{-i}))]\left(w_1 - \frac{1}{2}w_2\right) \geq 0.$$

Now if $n_{\phi(i)} = |\phi^{-1}(\phi(i))| > 1$ (there is more than one voter in district i), then $q_0 = q(s_i', \underline{1}_{-i})$; thus, one voter changing a vote does not affect the outcome. Hence, the above expression equals 0, and it follows that σ^t is a Nash equilibrium for G^t. If $n_{\phi(i)} = 1$ (there is a single voter in district i), then since $\sigma_i^t(1) = 1$ and $s_i' \leq 1$, it follows that $q_{0i} = \underline{1}$ and $q_i(s_i', 1_{-i}) = s_i' \leq 1$. In this case, one voter changing a vote changes the outcome. Hence, monotonicity of p implies that $[p_i(q_0) - p_i(q(s_i', \underline{1}_{-i}))] \geq 0$, and the last inequality holds if and only if

$$\left[w_1 - \frac{1}{2}w_2\right] = \frac{\mathscr{L}+1}{2\mathscr{L}} - \frac{1}{2\mathscr{L}} = \frac{1}{2} \geq 0.$$

Since all terms in the last expression are positive, this inequality holds; and it follows that s^t is a Nash equilibrium for G^t. This demonstrates that from the voter's point of view, changing a vote either does not change the outcome or changes the outcome in such a way as to make that voter worse off.

For $t \in \{LS\} \times Q^1$, we want to show that σ^t is a Nash equilibrium to the game with payoff function G^t, where $\sigma_i^t(t_{1i}) = 1$ for all $i \in L$. It suffices to show that for each $i \in L$, σ_i^t is at least as good as any pure strategy $s_i' \in S_i^t$. So,

$$G_i^t(\sigma^t) \geq G_i^t(\sigma_i', \sigma_{-i}^t)$$

$$\Leftrightarrow E_{\sigma^t}[u_i(\psi^t(s^t)) + \sum_{y \in \mathscr{T}} \pi^t(s^t)(y)v_i^y]$$

$$\geq E_{\sigma_{-i}^t}[u_i(\psi^t(s_i', s_{-i}^t)) + \sum_{y \in \mathscr{T}} \pi^t(s_i', s_{-i}^t)(y)v_i^y]$$

for all $s_i' \in S_i^t$. Using $\sigma_i^t(t_{1i}) = 1$, this can be reduced to

$$u_i(\psi^t(t_1)) + \sum_{y \in \mathscr{T}} \pi^t(t_1)(y)v_i^y \geq u_i(\psi^t(s_i', (t_1)_{-i}))$$

$$+ \sum_{y \in \mathscr{T}} \pi^t(s_i', (t_1)_{-i})(y)v_i^y.$$

Since $t_1 \in Q^1$, it follows that $\Sigma_{j \in L} t_{1j} > \mathscr{L}/2 \Rightarrow \pi^t(t_1)(LR, t_1) = 1$. So we get

$$v_i^{(LR,\, t_1)} \geq \pi^t(s_i', (t_1)_{-i})(LR, t_1)v_i^{(LR,\, t_1)}$$

$$+ \pi^t(s_i', (t_1)_{-i})(LR, q^*)v_i^{(LR,\, q^*)}.$$

Clearly, if $\pi^t(s_i', (t_1)_{-i})(LR, t_1) = 1$, legislator i is not pivotal and the above is an equality. So we consider the case when legislator i is pivotal, $\pi^t(s_i', (t_1)_{-i})(LR, t_1) \neq 1$. In this case, we must have $\Sigma_{j \in L}\, t_{1j} > \ell/2$ and $s_i' + \Sigma_{j \in L - \{i\}} t_{1j} < \mathscr{L}/2$. So $t_{1i} = 1$, and $s_i' = 0$. Thus, $\pi^t(s_i', (t_1)_{-i})(LR, q^*) = 1$, and the above inequality can be rewritten

$$v_i^{(LR,\, t_1)} \geq v_i^{(r,\, q^*)}$$

$$\Leftrightarrow (1-\theta)\left[p_i(t_1)w_1 + \frac{1}{2}\sum_{y \in \mathscr{L} - \{i\}} p_y(t_1)w_2\right] + c + \delta v_i^*$$

$$\geq (1-\theta)\left[q_i^* w_1 + \frac{1}{2}\sum_{y \in \mathscr{L} - \{i\}} q_y^* w_2\right] + c + \delta v_i^*$$

$$\Leftrightarrow p_i(t_1)w_1 + \frac{1}{2}(1 - p_i(t_1))w_2 \geq q_i^* w_1 + \frac{1}{2}(1 - q_i^*)w_2$$

$$\Leftrightarrow p_i(t_1)\left(w_1 - \frac{1}{2}w_2\right) \geq q_i^*\left(w_1 - \frac{1}{2}w_2\right)$$

$$\Leftrightarrow p_i(t_1) \geq q_i^*.$$

Now $p_i(t_1)$ is the probability that i will be selected, given that seniority is used and that $(\mathscr{L} + 1)/2$ members (including i) have seniority and $(\mathscr{L} - 1)/2$ do not have seniority. The seniority assumption implies that for all $q \in Q$, and $i, j \in L$, $q_i > q_i' \Rightarrow p_j(q) < p_j(q_i', q_{-i})$, $i \neq j$; that is, higher (lower) seniority for legislator i means that every other legislator now has a lower (higher) probability of being selected than the proposer. Now begin at q^* (assume every legislator has seniority) and remove seniority for $(\mathscr{L} - 1)/2$ legislators (not including i). At each step, p_i increases. Therefore, the last inequality is satisfied. Hence, σ^t is a Nash equilibrium for G^t.

For $t \in \{LS\} \times Q^0$, as above, we have

$$G_i^t(\sigma^t) \geq G_i^t(\sigma_i', \sigma_{-i}^t)$$

$$u_i(\psi^t(t_1)) + \sum_{y \in \mathscr{T}} \pi^t(t_1)(y)v_i^y \geq u_i(\psi^t(s_i', (t_1)_{-i}))$$

$$+ \sum_{y \in \mathscr{T}} \pi^t(s_i', (t_1)_{-i})(y)v_i^y$$

for all $s_i' \in S_i^t$. Since $t_1 \in Q^0$, it follows that $\Sigma_{j \in L} t_{1j} \leq \mathscr{L}/2 \Rightarrow \pi^t(t_1)(LR, q^*) = 1$. So we get

$$\Leftrightarrow v_i^{(LR,\, q^*)} \geq \pi^t(s_i', (t_1)_{-i})(LR, t_1)v_i^{(LR,\, t_1)}$$

$$+ \pi^t(s_i', (t_1)_{-i})(LR, q^*)v_i^{(LR,\, q^*)}.$$

Clearly, if $\pi^t(s_i', (t_1)_{-i})(LR, q^*) = 1$, legislator i is not pivotal and the above is an equality. So we consider the case when legislator i is pivotal, $\pi^t(s_i', (t_1)_{-i})(LR, q^*)) \neq 1$. In this case, we must have $\Sigma_{j \in L} t_{1j} \leq \mathscr{L}/2$ and $s_i' + \Sigma_{j \in L - \{i\}}\, t_{1j} > \mathscr{L}/2$. So $t_{1i} = 0$, and $s_i' = 1$. Thus, $\pi^t(s_i', t_{1-i})(LR, t_1) = 1$, and the above inequality can be rewritten

$$v_i^{(LR,\, q^*)} \geq v_i^{(LR,\, t_1)}$$

$$\Leftrightarrow (1-\theta)\left[q_i^* w_1 + \frac{1}{2}\sum_{y \in \mathscr{L} - \{i\}} q_y^* w_2\right] + c + \delta v_i^*$$

$$\geq (1-\theta)\left[p_i(t_1)w_1 + \frac{1}{2}\sum_{y \in \mathscr{L} - \{i\}} p_y(t_1)w_2\right] + c + \delta v_i^*$$

$$\Leftrightarrow q_i^* w_1 + \frac{1}{2}(1 - q_i^*)w_2 \ge p_i(t_1)w_1 + \frac{1}{2}(1 - p_i(t_1))w_2$$

$$\Leftrightarrow q_i^*\left(w_1 - \frac{1}{2}w_2\right) \ge p_i(t_1)\left(w_1 - \frac{1}{2}w_2\right)$$

$$\Leftrightarrow q_i^* \ge p_i(t_1).$$

But $p_i(t_1)$ is the probability that i will be selected, given that seniority is used and that $(\mathcal{L} - 1)/2$ members have seniority and $(\mathcal{L} - 1)/2$ (including i) do not have seniority. Using reasoning similar to that above, begin at q^* (assume no legislator has seniority) and add seniority for $(\mathcal{L} - 1)/2$ legislators (not including i). At each step, p_i decreases. Therefore, the last inequality is satisfied. Hence, σ^t is a Nash equilibrium for G^t.

For $t \in \{LV\} \times Q \times Z^1$, we want to show that σ^t is a Nash equilibrium to the game with payoff function G^t, where

$$\sigma_i^t(1) = \begin{cases} 1 & \text{if} \quad t_{1i} \ge \dfrac{1}{\mathcal{L}} \\ 0 & \text{if} \quad t_{1i} < \dfrac{1}{\mathcal{L}} \end{cases}$$

for all $i \in L$. It suffices to show that for each $i \in L$, σ_i^t is at least as good as any pure strategy $s_i' \in S_i^t$. So,

$$G_i^t(\sigma^t) \ge G_i^t(\sigma_i', \sigma_{-i}^t)$$

$$\Leftrightarrow E_{\sigma^t}[u_i(\psi^t(s^t)) + \sum_{y \in \mathcal{T}} \pi^t(s^t)(y)v_i^y]$$

$$\ge E_{\sigma_i^t}[u_i(\psi^t(s_i', s_{-i}^t)) + \sum_{y \in \mathcal{T}} \pi^t(s_i', s_{-i}^t)(y)v_i^y]$$

for all $s_i' \in S_i^t$. Since $t_1 \in Z^1$, $|\{j \in L: t_{1j} \ge w_2\}| > \mathcal{L}/2$. But $\sigma_i^t(1) = 1$ if $t_{1i} \ge 1/\mathcal{L} = w_2$. So, define $r \in \{0, 1\}^{\mathcal{L}}$ by $r_i = 1$ if $t_{1i} \ge w_2$ and $r_i = 0$ if $t_{1i} < w_2$. Then $\sigma(r) = 1$ and $\sigma_{-i}(r_{-i}) = 1$. Since $\Sigma_{i \in L}\, r_i > \mathcal{L}/2$, $\pi^t(r)(t_1, V) = 1$, and the above equation can be reduced to

$$u_i(x_0) + \sum_{y \in \mathcal{T}} \pi^t(r)(y)v_i^y \ge u_i(x_0) + \sum_{y \in \mathcal{T}} \pi^t(s_i', r_{-i})(y)v_i^y$$

$$\Leftrightarrow \sum_{y \in \mathcal{T}} \pi^t(r)(y)v_i^y \ge \sum_{y \in \mathcal{T}} \pi^t(s_i', r_{-i})(y)v_i^y$$

$$\Leftrightarrow v_i^{(V, t_1)} \ge \pi^t(s_i', r_{-i})(V, t_1)v_i^{(V, t_1)}$$

$$+ \pi^t(s_i', r_{-i})(LR, q^*)v_i^{(LR, q^*)}.$$

Clearly, if $\pi^t(s_i', r_{-i})(V, t_1) = 1$, legislator i is not pivotal and the above is an equality. So we consider the case when legislator i is pivotal, $\pi^t(s_i', r_{-i})(V, t_1) \neq 1$. In this case, we must have $\Sigma_{j \in L}\, r_j > \mathcal{L}/2$ and $s_i' + \Sigma_{j \in L - \{i\}}\, r_j < \mathcal{L}/2$. So $r_i = 1$, and $s_i' = 0$. Thus, $\pi^t(s_i', r_{-i})(LR, q^*) = 1$, and the above inequality can be rewritten

$$v_i^{(V, t_1)} \ge v_i^{(LR, q^*)} \Leftrightarrow (1 - \theta)t_{1i} + c + \delta v_i^* \ge v_i^*$$

$$\Leftrightarrow t_{1i} \ge \frac{1}{(1 - \theta)}[(1 - \delta)v_i^* - c].$$

$$\Leftrightarrow t_{1i} \ge \frac{1}{(1 - \theta)}\left[\left[\frac{1}{\mathcal{L}}(1 - \theta) + c\right] - c\right].$$

$$\Leftrightarrow t_{1i} \ge \frac{1}{\mathcal{L}}.$$

But $s_i^t = 1 \Rightarrow t_{1i} \ge w_2 = 1/\mathcal{L}$. Hence, the above inequality holds; and we have shown that $G_i^t(\sigma^t) \ge G_i^t(\sigma_i', \sigma_{-i}^t)$, so σ^t is a Nash equilibrium for G^t.

For $t \in \{LV\} \times Q \times Z^0$, define r as above. Since $t_1 \in Z^0$, $|\{j \in L: t_{1j} \ge w_2\}| \le \mathcal{L}/2$, we get $\Sigma_{i \in L}\, r_i \le \mathcal{L}/2$, implying $\pi^t(r)(LR, q^*) = 1$. Then arguing as above,

$$G_i^t(\sigma^t) \ge G_i^t(\sigma_i', \sigma_{-i}^t)$$

$$\Leftrightarrow v_i^{(LR, q^*)} \ge \pi^t(s_i', r_{-1})(V, t_1)v_i^{(V, t_1)}$$

$$+ \pi^t(s_i', r_{-i})(LR, q^*)v_i^{(LR, q^*)}$$

for all $s_i' \in S_i^t$. Clearly, if $\pi^t(s_i', r_{-i})(LR, q^*) = 1$, legislator i is not pivotal and the above is an equality. So we consider the case when legislator i is pivotal, $\pi^t(s_i', r_{-i})(LR, q^*) \neq 1$. In this case, we must have $\Sigma_{j \in L}\, r_j < \mathcal{L}/2$ and $s_i' + \Sigma_{j \in L - \{i\}}\, r_j > \mathcal{L}/2$. So $r_i = 0$, and $s_i' = 1$. Thus, $\pi^t(s_i', r_{-i})(V, t_1) = 1$, and the above inequality can be rewritten

$$v_i^{(LR, q^*)} \ge v_i^{(V, t_1)} \Leftrightarrow v_i^* \ge (1 - \theta)t_{1i} + c + \delta v_i^*$$

$$\Leftrightarrow \frac{1}{(1 - \theta)}[(1 - \delta)v_i^* - c] \ge t_{1i}.$$

$$\Leftrightarrow \frac{1}{(1 - \theta)}\left[\left[\frac{1}{\mathcal{L}}(1 - \theta) + c\right] - c\right] \ge t_{1i}.$$

$$\Leftrightarrow \frac{1}{\mathcal{L}} \ge t_{1i}.$$

But $s_i^t = 0 \Rightarrow t_{1i} < w_2 = 1/\mathcal{L}$. Hence, the above inequality holds; and it follows that σ^t is a Nash equilibrium for G^t.

For $t \in \{LP\} \times Q \times L$, we want to show that σ^t is a Nash equilibrium to the game with payoff function G^t, in which

$$\sigma_t^t = \frac{1}{|\Omega_t|}\Sigma_{\omega \in \Omega_t}\, \delta_{z_t}(\omega),$$

where $\Omega_t = \{\omega \in \{0, 1\}^{\mathcal{L}}: \Sigma_i \omega_i = (\mathcal{L} + 1)/2,\ \omega_t = 1\}$ and $z_t: \Omega_t \to \mathbb{R}^{\mathcal{L}}$ is defined by

$$z_{ti}(\omega) = \begin{cases} \dfrac{\mathcal{L} + 1}{2\mathcal{L}} & \text{if} \quad i = t \\ \dfrac{1}{\mathcal{L}} & \text{if} \quad i \neq 1,\ \omega_i = 1 \\ 0 & \text{otherwise} \end{cases}$$

for all $i \in L$. It suffices to show that σ_t^t is at least as good as any pure strategy $s_i' \in S_i^t$. So, for all $s_t' \in S_t$,

$$G_t^t(\sigma^t) \geq G_t^t(\sigma_t', \sigma_{-t}^t)$$

$$\Leftrightarrow E_{\sigma^t}[u_t(\psi^t(s^t)) + \sum_{y \in \mathcal{T}} \pi^t(s^t)(y)v_t^y]$$

$$\geq E_{\sigma_{-i}^t}[u_t(\psi^t(s_t', s_{-t}^t)) + \sum_{y \in \mathcal{T}} \pi^t(s_t', s_{-t}^t)(y)v_t^y]$$

$$\Leftrightarrow u_t(x_0) + \frac{1}{|\Omega_t|} \textstyle\sum_{w \in \Omega_t} \displaystyle\sum_{y \in \mathcal{T}} \pi^t(z_t(\omega), \underline{0}_{-t})(y)v_t^y$$

$$\geq u_t(x_0) + \sum_{y \in \mathcal{T}} \pi^t(s_t', \underline{0}_{-t})(y)v_t^y$$

$$\Leftrightarrow \frac{1}{|\Omega_t|} \textstyle\sum_{w \in \Omega_t} v_t^{(LV,\, z_t(\omega))} \geq v_t^{(LV,\, s_t')}.$$

But now, for all $\omega, \omega' \in \Omega_t$, $z_{tt}(\omega) = z_{tt}(\omega')$. So, writing $z_{tt} = z_{tt}(\omega)$, then the above inequality becomes

$$\frac{1}{|\Omega_t|} \textstyle\sum_{w \in \Omega_t} v_t^{(LV,\, z_t(\omega))} = (1 - \theta)z_{tt} + c + \delta v_t^* \geq v_t^{(LV,\, s_t')}$$

Now if $s_t' \in Z^0$, which means that the proposal will not pass, then $v_t^{(LV,\, s_t')} = v_t^*$. So the above inequality becomes

$$(1 - \theta)\frac{\mathcal{L} + 1}{2\mathcal{L}} + c \geq (1 - \delta)v_t^* = (1 - \theta)\frac{1}{\mathcal{L}} + c$$

$$\Leftrightarrow \frac{\mathcal{L} - 1}{2\mathcal{L}} \geq 0 \Leftrightarrow \mathcal{L} \geq 1.$$

Since this inequality holds, σ^t is a Nash equilibrium for G^t in this case. On the other hand, if $s_t' \in Z^1$, the proposal is one that will pass; then in order to have $|\{j \in L: s_{tj} > w_2\}| \geq \mathcal{L}/2$, we must have $s_{tt}' \leq s_{tt}^t$. But then

$$\frac{1}{|\Omega_t|} \textstyle\sum_{w \in \Omega_t} v_t^{(LV,\, z_t(\omega))}$$

$$= (1 - \theta)s_{tt}^t + c + \delta v_t^* \geq (1 - \theta)s_{tt}' + c + \delta v_t^* = v_t^{(LV,\, s_t')}.$$

Hence, σ^t is a Nash equilibrium for G^t.

Notes

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1. There have been partial attempts in this direction. Austen-Smith and Banks (1988) develop a full-equilibrium model of voter and legislative behavior in a parliamentary system. However, their model is not dynamic, since it deals with a one-shot game. Kramer (1977), Baron (1989), and Ferejohn (Baron and Ferejohn 1989) have developed dynamic models of policy formation and legislative organisation; but these models are not full-equilibrium, since they do not explicitly consider voter and legislative interactions.

2. On the issue of seniority, there has been remarkably little formal work in the political science literature. One exception is Shepsle and Nalebuff (1990), which develops a model explaining the existence of seniority systems in the group provision of public or private goods. Their explanation is based on a model of overlapping generations, in which agents need to have incentives to participate throughout their lifetime. This explanation does not depend on any characteristics of the group that are unique to legislative bodies and hence is equally applicable to firms and legislative bodies. Although there has not been a lot of explicit work on seniority, there has been a substantial body of formal work looking at the role of specialized committees in legislative organization (e.g., Gilligan and Krehbiel 1990; Shepsle 1979).

3. We model the infinitely repeated game as a stochastic game and only consider stationary solutions, thus avoiding the usual problems associated with the folk theorems in repeated games.

4. It is important to note that in our formulation, the seniority system only matters on the initial proposal. An interesting variation to consider would be the case in which seniority counts not only on the first proposal but on all successive proposals, as well. We believe that our formulation makes sense for two reasons. First, it captures an aspect of how congressional rules operate—namely, seniority is embodied in the committee system, which gives higher-than-average influence to ranking committee members to specify the proposed legislation. But if a majority of the legislators oppose a committee proposal on the floor, then the committee effectively loses its power, and the proposal of the committee can be amended by the full legislature at will. Second, suppose one defines the status quo to be fair division (each district gets $1/\mathcal{L}$). Then our model is equivalent to a model in which the failure of a proposal leads to a reversion to the status quo.

5. While this is not completely realistic, it captures the idea that voters can punish their representatives who they feel are not acting in their best interests. Our formulation allows more limited punishments than would be the case if voters could remove the legislator from office permanently.

6. Elements of $\mathcal{T}$ will be denoted by t. If t has two components, then we write $t = (t_0, t_1)$. The reader is cautioned that t is a variable indexing the games and thus can represent different things in different games.

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THEORY AND PRACTICE: ALFARABI'S *PLATO* REVISITED

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According to Leo Strauss, knowledge of the best way of life is crucial to political philosophy. In "Farabi's Plato,*" Strauss asks, assuming that the theoretical life can be known to be the best way of life, what is the status of this knowledge? Is the knowledge of the best way of life itself theoretical knowledge or practical knowledge? Without a coherent answer to this question, we cannot be certain that we know what we mean when we claim to know that philosophy is the best way of life. Strauss answers clearly the question about the status of the knowledge of the best way of life by affirming that it is practical, not theoretical, knowledge. For a variety of reasons, this answer is not persuasive in the form in which Strauss gives it.*

One of the most conspicuous themes of the work of Leo Strauss is the relationship between theory and practice. Whether Strauss regards this relationship as the highest, or one of the highest, themes of philosophy, he certainly sees it as one of the most urgent or necessary (1953, 162–63). In a recent book on Strauss, Heinrich Meier draws attention to the relatively early article "Farabi's *Plato*" (Strauss 1945), in which Strauss directly and specifically addresses this subject (Meier 1988a, 95–96). Despite the title, the article gives a strikingly explicit account not only of the views of Farabi, or Alfarabi, but also of Strauss himself, at least at that time, on what we can know about the dignity of philosophy as a way of life—the theoretical life—and about the practical alternatives to it.

Alfarabi (870–950 C.E.) was an Islamic philosopher with whom Strauss concerned himself for at least 20 years.[1] Strauss was interested in Alfarabi as a predecessor of Maimonides and as a representative of a kind of rationalism distinct from modern rationalism. While Strauss saw modern rationalism as somehow in the service of the conquest of nature and, as such, having an essentially practical aim, Alfarabi, as interpreted by Strauss, presented an alternative rationalism that was essentially theoretical or contemplative. Strauss, like Nietzsche, seems to have diagnosed a crisis of modern rationalism; but where Nietzsche is forced to turn away from reason in search of a cure, Strauss seeks a remedy in a return to an earlier form of rationalism. Meier finds in "Farabi's *Plato*" a significant statement of the philosophical position Strauss continued to elaborate over the following quarter of a century. Since Alfarabi's philosophy is of interest in its own right, we have two reasons for examining Strauss's early essay.

The full title of the short work that I, following Strauss, shall call the *Plato* is *The Philosophy of Plato, Its Parts, the Ranks of Order of Its Parts, from the Beginning to the End*. It is part 2 of the three-part *Philosophy of Plato and Aristotle*, part 1 being *The Attainment of Happiness* and part 3, *The Philosophy of Aristotle*.[2] Strauss focuses his attention on the *Plato* because he believes that Alfarabi has been more candid in that work (where Alfarabi speaks through the mouth of Plato) than in works where he speaks in his own name—for example, *The Attainment of Happiness* (Strauss 1945, 371–75).

I shall consider three topics, with a view to reflecting on the proper relation between knowledge or science on the one hand and human life or practice and its aims, on the other: (1) Does the knowledge of the best way of life belong to what Strauss calls philosophy "in the precise sense" (1945, 365) or does it belong to moral and political philosophy? (2) How do Alfarabi and/or Strauss view the status of Plato's philosopher–king? Is political ability ultimately identical with philosophic ability? Could the two ever be combined in one individual? (3) Can philosophy change the world, as Marx hoped? Or does the philosopher necessarily exist in an imperfect world, now and always, as Strauss's Alfarabi contends? Is philosophy essentially practical or theoretical?

While I try to review several aspects of Strauss's argument, it may be helpful to state the most significant conclusion of the inquiry at this point. For Strauss, much depends on the question, What is the right way of life? The standard by which to judge the best political order depends on the right, or best, way of life. The practical question about the best political order can be answered only by answering the question about the best way of life. The question about the best way of life is also a practical question. It is a deeper—a more fundamental—practical question than the question about the best political order. For Strauss, the right or best way of life turns out to be philosophy "in the precise sense" of theoretical, not practical, inquiry. Philosophy as the right or best way of life is not a value. It is not an arbitrary belief, conviction, preference, or life-style. According to Strauss, philosophy can be known to be the best way of life. In "Farabi's *Plato*," Strauss does not try to prove that philosophy is the best way of life. Rather, he devotes his attention to the question, Given that philosophy can be known to be the right way of life, what is the status of this knowledge? Is this knowledge itself theoretical knowledge or practical knowledge? Without a coherent answer to this question, we cannot be certain that we know what we mean when we claim to know that philosophy is the best way of life. This knowledge is crucial for political philosophy as it is understood by both Strauss and Alfarabi. Strauss answers the question clearly by affirming that the knowledge of the best way of life is itself practical, not theoretical, knowledge. I give reasons why this position is not persuasive.

In Strauss's classification of the forms of knowledge, practical knowledge is of lower rank or dignity than theoretical knowledge. It is hard to avoid the conclusion that practical knowledge is of lower cognitive status. Given that Alfarabi treats theoretical knowledge as certain knowledge, one must wonder whether practical knowledge is not less than certain. The consequences are obviously serious if the claim that philosophy is the best way of life is not more certain or true than rival claims on behalf of piety, honor, or pleasure. We are forced at least to raise the question how the classification of the knowledge of the best way of life as merely practical avoids decaying into the view that such "knowledge" is in fact a value judgment, preference, or mere conviction. It may seem ludicrous to suggest that Strauss ascribes to Alfarabi—and himself subscribes to—a position that collapses into a value judgment; but it may be equally ludicrous not to raise this question. Nietzsche would have us believe that all philosophers make such judgments, and Rosen singles out Strauss as being on this point a Nietzschean who regards philosophy as an act of the will (Rosen 1987, 110–11, 122–23, 127, 137). Clearly, Strauss does not intend to make philosophy an act of the will (see my earlier work, Colmo 1990). It is equally clear, however, that Strauss's own position collapses if he cannot give a coherent account of the knowledge by which he knows that philosophy is the best way of life. That account, as Strauss presents it in the totality of his writings, is by no means obvious. This problem is the motive for the critique of Strauss's view I now offer, of which the first section contains the most important results. I subsequently draw out further implications of Strauss's position and, in some cases, point to ways in which alternatives to Strauss's interpretation of Alfarabi suggest alternative answers to the basic philosophic questions at issue.

PHILOSOPHY PROPER AND POLITICAL PHILOSOPHY

Alfarabi makes it plain that the practical or political art (we would call it political philosophy) is concerned with discovering and providing for the "desired way of life" (1969, 60). Let us begin with the question whether the desired way of life, the life that makes man perfect or happy, can be the virtuous way of life (p. 54)? More specifically, can the virtues practiced in mosques and temples (i.e., the virtues inculcated by religion) make human beings perfect and happy (p. 61)?

Strauss argues persuasively that for Alfarabi, religion and the virtues based on religion do not provide the desired way of life (1945, 373). Strauss contends, more openly than Alfarabi, that the moral life (including, of course, the moral life based upon religion) cannot be the desired way of life because the moral life as such does not allow one to ask why one should be moral: "The moral life consists of the submission to the demands of honor and duty without reasoning why" (p. 388). The premise of Strauss's Alfarabi is that the desired way of life must be a reasoned way of life or a way of life of which a reasoned account can be given. The desired way of life cannot be based on mere choice be that choice inspired, mysterious, arbitrary, or irrational. The way of life acceptable to philosophy must be justified by reason. But morality that is rationally justified is no longer morality. Moral actions must be "choiceworthy for their own sake," that is, because they are moral, not because they are rationally dictated (Strauss 1964, 27). The virtuous way of life cannot be the desired way of life because morality as such is inherently unphilosophic. Moral philosophy, if such a thing is possible, cannot be a part of philosophy in the precise sense. Of course, it is important to add that nothing stops Strauss or anyone else from moving outside the assumption made here in order to seek a morality that is rationally grounded.

The moral life by itself cannot be the best way of life because it is unphilosophic. That the best—or, as Alfarabi says, desired—way of life, is the philosophic becomes plain in Alfarabi's discussion of the Socratic way of life. Alfarabi asserts, in terms much harsher than those used by Plato's Socrates, that the unexamined life is not worth living (1969, 63–64). The philosophic life is the only desirable life. But what is the philosophic life? According to Socrates, philosophy includes the study of the moral, political, or practical things. For this reason, even Socrates does not escape Alfarabi's implicit criticism, according to Strauss (1945, 363–65, 382–83). The best way of life is not the one that examines practical matters. Man's perfection is to be found in the knowledge of "the substance of each of the beings," "the beings," or the "natural beings" (Alfarabi 1969, 54, 56, 60, 65). The way of life devoted to the pursuit of such knowledge is what Strauss calls philosophic in the precise sense (1945, 365). Whereas the moral or practical things are the products of human action or choice, philosophy in the strict sense seeks knowledge only of the natural things, which are not products of any human knowing, making, action, or choice. Philosophy in the precise sense is strictly theoretical.

If we go this far with Alfarabi, then we arrive at a pressing difficulty. The "difference between the truly virtuous way of life and all other ways of life is based, not on a difference of purpose, of quality of the will, but on a difference of knowledge" (Strauss 1945, 388–89). Philosophy, as the life of reason, cannot be based upon an arbitrary choice or an act of the will. The philosophic way of life must be *known* to be superior to the moral or political life. The difficulty arises when we try to classify the kind of knowledge by which we know that philosophy is the right, or desired, way of life. Alfarabi carefully distinguishes between the knowledge of the beings (whatever these may be) and the knowledge of the ways of life (1969, 56; Strauss 1945, 365). A way of life is not a being (Strauss 1945, 389). Strauss does not try to avoid the obvious conclusion that even knowledge of the best way of life is not philosophic or theoretical knowledge; knowledge of the best way of life is supplied by the highest practical art (p. 365).

Strauss's interpretation would eventually have to be brought into harmony with three works in which Alfarabi asserts or implies that "only the theoretical rational faculty can attain real knowledge of what happiness is" (cited in Galston 1990, 69). This amounts to saying, contra Strauss, that knowledge of the best way of life is theoretical knowledge. In general, Galston seems to take the view that Alfarabi blurs the traditional distinction between theoretical and practical knowledge so that the former includes much of the latter.

The assertion that the knowledge of the best way of life is not itself philosophic knowledge (strictly speaking) goes well beyond the assertion, discussed earlier, that the virtuous or moral way of life is not, as such, philosophic. This assertion is clearly based on philosophic knowledge. This knowledge in no way implies, however, that philosophy itself cannot be philosophically known to be the best way of life. Nor, certainly, does it imply that knowledge of philosophy as the best way of life is itself moral "knowledge," rather than philosophic knowledge.

Strauss moves in a different direction. In keeping with the differentiation of theoretical and practical knowledge, he suggests that it would be foolish to assume that philosophy exhausts itself in the investigation of the relation between philosophy and happiness (1945, 363). In other words, it would be foolish to assume that philosophy exhausts itself in the quest for the best way of life or that the philosopher has no leisure for anything other than this quest (1964, 21, 29). Indeed, the quest for the best way of life and the knowledge of that way of life are "strictly speaking merely preliminary" (1945, 366). The study of "the human or political meaning of philosophy" does not belong "to the same level" as philosophy proper (pp. 366, 368). One is even a little surprised to find that in the course of explaining Alfarabi's remarks on the limitations of Plato's Socrates as compared with Plato himself, Strauss says that Socrates was "merely a moral philosopher" who "neglected natural philosophy" (1945, 383; see Alfarabi 1969, 66–67). The moral philosophy that is disparaged here seems to include reflection on philosophy as the best way of life; Socratic reflection surely included this subject.

Since it is not philosophy strictly speaking, what is the status of political philosophy understood as the investigation of the human meaning of philosophy? Strauss cannot deny that practical knowledge in this extended sense is really knowledge. The discovery of the philosophic life as the best way of life cannot be reduced to the level of an arbitrary choice or an act of the will. The superiority of the philosophic life must be known, not willed (1945, 389). This point is central to Strauss's position.[3] Hence, Strauss's final formulation of the problem in "Farabi's *Plato*" seems to be that knowledge of the right way of life can "be described as philosophic since only the philosopher is competent to elaborate that question and to answer it" (p. 366). Such knowledge, however, is of a lower level than theoretical knowledge; even the highest practical knowledge is merely preliminary. Moreover, that the knowledge of the ways of life is not a proper subject of theory (because, to repeat, ways of life are not beings) is itself a theoretical insight (p. 389). To summarize in a way that may be more precise than clear, the knowledge that the theoretical life is the best way of life can be theoretically known to be merely practical knowledge.

Is merely practical knowledge certain knowledge? According to Alfarabi, theoretical knowledge is certain knowledge (Alfarabi 1969, 13; Mahdi 1973, 7–9). Strauss says that Alfarabi identifies philosophy with "the art of demonstration" (1945, 364). But in his *Philosophy of Aristotle*, Alfarabi calls the art of demonstration "the art of certainty" (1969, 87). If the distinguishing feature of theoretical knowledge is its certainty, is practical knowledge somehow uncertain? If it is, then how does philosophy ward off the rival claims of other ways of life? Ways of life that are openly based on faith, belief, or conviction would seem to be, at any rate, more consistent than a philosophic life dedicated to knowledge but itself based on faith or conviction (Strauss 1953, 75). The least one can say is that in "Farabi's *Plato*," Strauss defines a position that entails this serious dilemma without, however, acknowledging it or dealing with it.

It is interesting to compare Strauss's 1945 essay with his later writings. In *The City and Man*, Strauss raises the question whether it is possible to know that the philosophic life is the best life:

> Socrates could not know this if he did not know that the only serious alternative to the philosophic life is the political life and that the political life is subordinate to the philosophic life: political life is life in the cave which is partly closed off by a wall from life in the light of the sun; the city is the only whole within the whole or the only part of the whole whose essence can be wholly known. (1964, 29)

Knowledge of the best way of life comes from understanding the relationship between the philosophic life and political life, or life in the cave. But Strauss does not tell us whether this understanding is theoretical or practical, though the last clause in the passage just quoted seems to invite the conclusion that understanding of politics is a very high, if not the highest, kind of understanding. Does Strauss here mean to suggest the possibility that practical knowledge is more certain than theoretical knowledge? From this perspective, there would seem to be no longer any reason to exclude practical knowledge (as being the most certain knowledge) from philosophy in the precise sense.

In an essay first published toward the end of his life (1971), "Philosophy as Rigorous Science and Political Philosophy," Strauss returned to the issue of this relationship (1983, chap. 1). The closest equivalent to what Strauss calls moral philosophy in "Farabi's *Plato*" is what he calls *Weltanschauungsphilosophie* (ibid.)—assuming it is not closer to what Farabi calls the religious investigation of the beings (1945, 373, n. 42). *Weltanschauungsphilosophie* conceptualizes religious, aesthetic, ethical, political, and practical–technical experience. Although this kind of philosophy "presents the relatively most perfect solution of the riddles of life and the world," it is nevertheless to be distinguished from philosophy as rigorous science for the same reason that Alfarabi distinguishes between moral philosophy and theoretical philosophy. *Weltanschauungsphilosophie* is the "philosophy" of the cave because in one way or another it presents, as true, things about which we cannot be certain, no matter how much we may want that certainty. If this is so, then the thought from *The City and Man* can be restated by saying that Socrates can know that the philosophic life is the best way of life only because he can know that philosophy as rigorous science is unqualifiedly superior to *Weltanschauungsphilosophie.* Moreover, the superiority of philosophy as rigorous science seems to depend on its more stringent standard of certainty.

Our primary question now becomes whether knowledge of the competing claims of the two kinds of philosophy is itself practical knowledge or theoretical knowledge. This time Strauss gives us a fairly straightforward answer:

> Reflection on the relation of the two kinds of philosophy obviously belongs to the sphere of philosophy as rigorous science. It comes closest to being Husserl's contribution to political philosophy. . . . In order to see the relation between philosophy as rigorous science and the alternative to it clearly, one must look at the political conflict between the two antagonists, i.e., at the essential character of that conflict. (1983, 36–37)

I take it that knowledge of the essential character of the conflict between the two kinds of philosophy includes knowledge of the superiority of philosophy as rigorous science. According to Strauss, it is only through such knowledge that Socrates could know the philosophic life to be the best life. Finally, Strauss makes it clear that such knowledge (knowledge of the best way of life) belongs to the sphere of philosophy as rigorous science. But it is equally clear that in "Farabi's *Plato*" this is not the case. There, reflection on the practical issue of the relation between theoretical philosophy and practical philosophy or reflection on the human meaning of philosophy does not belong to philosophy strictly speaking or to philosophy as rigorous science.

"Farabi's *Plato*" leaves the reader in some uncertainty concerning the status of reflection on the best way of life. Knowledge of the best way of life, the philosopher's self-knowledge (1945, 366), seems to be neither knowledge strictly speaking, nor religion, nor morality. But if it is none of these, what is it? Plato suggests that what lies between knowledge and ignorance is opinion or right opinion. There is considerable doubt as to whether even right opinion—much less, mere opinion—"can nevertheless be described as philosophic" (ibid.). Yet Strauss cannot settle for less if the superiority of the philosophic life is to be known, rather than merely willed. Such difficulties may have moved Strauss to elevate political philosophy—understood as comprehending the question of the best way of life—to the level of philosophy as rigorous science. While this clarifies the epistemological status of political philosophy, it seems paradoxical. How can the knowledge of the best way of life fail to be a kind of practical knowledge? Alfarabi, for one, always identifies the knowledge of the desired way of life as practical knowledge. Strauss insists upon this point and Miriam Galston reinforces it when she argues that for Alfarabi, the practical differs from the theoretical not in subject matter but in purpose, namely, a concern with human happiness (1990, 55, 69). Even if we reclassify knowledge of the best way of life as theoretical knowledge, some problems remain.

In making use of "Farabi's *Plato*," Meier runs the risk (as he is no doubt aware) that Strauss may have partially changed his mind in later years. But the risk may not be as great as we have made it seem. The difference between "Farabi's *Plato*" and "Philosophy as Rigorous Science and Political Philosophy" hides a basic agreement concerning one of the problems Strauss must surely have had in mind when he spoke of "the fundamental and permanent problems" (1959, 39). What is consistent in the two essays is the supremacy of theoretical knowledge over self-knowledge or practical knowledge.[4] The knowledge of the best way of life is either practical and therefore the lowest level of philosophy (as in the former essay) or given full theoretical status (as in the latter essay).[5] Strauss never adopts the view that philosophy proper consists of theoretical and practical philosophy on an equal level (1945, 366). He never suggests that if knowledge of the best way of life is to be ranked higher among levels of knowledge then this should be accomplished by elevating the rank of practical knowledge, rather than by removing the knowledge of the best way of life from the level of the practical altogether. The two essays share a fundamental assumption or tendency.

THE PHILOSOPHER–KING

Strauss's interpretation is based on the distinction that Alfarabi obviously makes between the science of the beings and the science of the ways of life (Alfarabi 1969, esp. 56). But Alfarabi makes another statement that is difficult to reconcile with the implications of this distinction. Alfarabi explicitly identifies philosophy with the royal art. Indeed, he follows Plato in making human happiness dependent on the existence of a philosopher–king. The passage in question is worth quoting in full as Strauss translates it:

> [According to Plato,] the *homo philosophus* and the *homo rex* are the same thing. (1) [According to Plato,] each of the two (*sc.* the philosopher and the king) is rendered perfect by one function and one faculty. (2) [According to Plato,] each of the two (*sc.* the philosopher and the king) has one function which supplies the science desired from the outset and the way of life desired from the outset; (3) each of the two (*sc.* functions) produces in those who take possession of it, and in all other human beings, that happiness which is truly happiness. (Strauss 1945, 367)

For Strauss, none of the statements in the passage he quotes strictly identifies philosophy with kingship or the royal art. The statements do no more than assert that philosophy and the royal art exist together in the same man; the two things are not identical.[6] Because the third statement goes so far, however, as to assert that philosophy produces the happiness of all human beings, it does "practically" identify philosophy with the royal art (p. 367). But Strauss regards this "extravagantly philanthropic remark," when taken literally, to be a "sheer absurdity" (p. 378). According to Strauss, such an extravagant remark on Alfarabi's part has three purposes.

First, if philosophy is the best way of life or the only true happiness, most human beings would be excluded from happiness (Strauss 1945, 381). The identification of philosophy with kingship helps to avoid or hide this conclusion, since the philosopher–king makes possible the happiness of "all," or of the nonphilosophic many.

Second, the royal art is the practical art that provides the desired, or right, way of life (Alfarabi 1969, 60). From this point of view, the identification of philosophy with kingship is a "pedagogic device" leading the reader toward the view that philosophy supplies not only the science of the beings but also the right way of life (1945, 370). Philosophy is self-sufficient. If philosophy is itself the royal or political art, then it is in no way dependent upon politics for guidance toward the desired way of life.

Alfarabi's third point, as Strauss sees it, derives from the fact that the philosopher necessarily lives in political society and that his relations with his nonphilosophic fellow citizens are "naturally difficult" (1945, 382). The philosopher needs a kind of political art in order to deal with these difficulties. Another way to say this is that philosophy, or wisdom, on the one hand and self-knowledge, or moderation (*sophrosyne*), on the other "cannot be separated from each other" (p. 366). The recognition that philosophy and the royal art cannot be separated does not require, however, that we take the identification of philosophy with kingship literally.

Alfarabi, Strauss says, "leaves no doubt . . . that philosophy and the royal art are coextensive" (1945, 368). Philosophy and the royal or political art necessarily exist together in the same human being: "A human being cannot acquire the specific art of the philosopher without at the same time acquiring the specific art of the king and *vice versa*" (p. 367). Strauss thinks that this view is an acceptable *alternative* to the view that philosophy and kingship are identical.

Strauss provides the following gloss on statements 2 and 3 of the quoted passage in which Alfarabi describes the relationship between philosophy and the royal art:

> The function of the philosopher supplies by itself both the science of the beings and the right way of life and thus produces true happiness in both the philosophers and all other human beings; the function of the king supplies by itself both the science of the beings and the right way of life and thus produces true happiness in both the kings and all other human beings. (1945, 367)

This restatement of Alfarabi's text brings out the fact that the text is quite compatible with the view that the function of the philosopher and the function of the king are two separate functions. Since there are two distinguishable functions, Strauss is able to preserve "the difference of level between philosophy proper and moral or political investigations" (p. 368). The reference to "all human beings" falls under the heading of philanthropic extravagance explained above.

Strauss's restatement, however, does not bring out the fact that the function of the philosopher is "one function"—or "a single skill," as Mahdi translates it (Alfarabi 1969, 60)—just as the royal function is "one function." Even allowing for some kind of distinction between the philosopher and the king, it is still the case that *one* function or a *single* skill supplies both the desired science (the science of the beings, according to Strauss) and the desired way of life.[7] We would have to admit that the one function or the single skill somehow combines theory and practice. Since Strauss sees theory as being on a different level from practice, the notion that one function or a single skill supplies the desired theory and the desired practice requires clarification.

Strauss offers the following explanation. Philosophy, while primarily and essentially directed toward the science of the beings, "cannot be exercised fully without producing the right way of life"; kingship cannot be exercised fully without producing the science of the beings, but it remains "primarily and essentially concerned with the right way of life" (1945, 368). In the best case, philosophy and kingship cannot be separated; but they are not identical.

It is certainly not unfair to ask why, if philosophy is essentially concerned with one thing and kingship is essentially concerned with another thing, Alfarabi emphasizes that the single function–philosophy–is concerned with two things (one theoretical and one practical) and the single function–kingship–is likewise concerned with two things? To speak only of philosophy, it can supply both the desired knowledge and the desired way of life only if it supplies both theory and practice. Strauss himself draws the necessary conclusion from his own premises: "The identification of philosophy as the highest theoretical art with the royal art as the highest practical art can be literally valid . . . if contemplation itself is the highest form of action" (1945, 386).

Strauss does not indicate whether the single function or art that supplies both the desired science and the desired way of life also supplies the knowledge of the desired way of life. With this question, I return to the first topic of the essay. Alfarabi's (1969) ambiguous expression *desired science* or *desired knowledge* certainly seems to suggest the possibility that the desired science might include not only the science of the beings, mentioned by Strauss in his gloss to the text but also "the science of the ways of life," which Strauss mentions elsewhere (1945, 365). The knowledge of the right way of life would certainly seem to be some part of the desired science. The least one could say is that while Strauss emphasizes the difference of level between the theoretical and the practical, Alfarabi emphasizes the unity of the function or skill that supplies both theory and practice.

For Strauss, a unity of theory and practice can occur only on the plane of the theoretical: theory or contemplation itself is the highest form of action. There is a place where Alfarabi, in a somewhat different way, considers the possibility of a man who combines theoretical science and practical science; but he does not there say whether the combination itself ought to be regarded as theoretical or practical (1969, 66). On the latter point, there is only one explicit statement in the *Plato*: Alfarabi treats the practical arts as the arts that combine knowledge and action (p. 58). He is silent about the possibility of a combination of knowledge and action on the level of the theoretical. Indeed, shortly before the passages suggestive of a philosopher–king, Alfarabi lists three possibilities: a scientific art that supplies the desired knowledge, a practical art that supplies that knowledge, or a practical art that supplies the desired way of life (p. 59). While Alfarabi at least entertains the possibility of a practical art that supplies the desired knowledge, he does not entertain the possibility of a scientific or theoretical art that supplies the desired way of life.[8]

Statements by Alfarabi relevant to the subject of the philosopher–king do more than open the question whether theory is not itself the highest form of practice. As we have seen, under Strauss's scrutiny, Alfarabi's statements also raise the question whether philosophy is coextensive with practice in the sense of political kingship. Could both functions exist in the same human being? Would the excellence of the philosopher complement that of the king and vice versa? Or would the attempt to perform both functions diminish the performance of each in itself? In the latter case, is division of labor—one person, one function—superior to any attempt at the coexistence of the two functions in one human being? Or does the perfection of any one function fall short of being the perfection of the whole person?

As Strauss explains (and as we have seen), Alfarabi "leaves no doubt" that in the best case philosophy and kingship coexist in the same human being. This does not mean that the best philosopher must rule a specific city or nation but, rather, that the best human being combines the two capacities. Strauss goes so far as to suggest that the philosopher and the king have the same nature (1945, 368, n. 28).

Here, again, it is instructive to compare "Farabi's *Plato*" with a later statement by Strauss. In "Xenophon's *Anabasis*," Strauss makes the following observation:

> Xenophon stands somewhere in between the older Cyrus [a manly or political man] and Socrates. By this position he presents to us not a lack of decisiveness but the problem of justice: justice requires both the virtue of a man . . . and the virtue of Socrates; the virtue of the [manly or political] man points to Socratic virtue and Socratic virtue requires as its foundation the virtue of the man; both kinds of virtue cannot coexist in their plenitude in one and the same human being. (1983, 128)

In "Farabi's *Plato*," Strauss presents the coexistence of political and philosophic excellence in one human being as an alternative to the identification of those two excellences. One cannot acquire the one "without at the same time" acquiring the other (1945, 367). In his essay on Xenophon, he presents the two virtues as incompatible "in their plenitude." These two statements contradict each other. What remains constant between the two essays is the superiority of the virtue of Socrates, the theoretical human being, to the virtue of Cyrus, the political being. Indeed, the gulf between the two has widened from the first essay to the second; theory remains supreme.

Galston presents evidence that Alfarabi shared the view espoused by Strauss in his article on Xenophon (1990, 84). The activities presupposed by practical perfection could be understood "to be incompatible with, and possibly to undermine, the activity comprising theoretical perfection." This problem arises because of passages in Alfarabi in which theoretical perfection requires the complete transcendence of the body. On balance, Galston (1990, 53–94, esp. 56 and 91, n. 69) rejects this view in favor of what she calls the comprehensive understanding of happiness, wherein theoretical and practical activity are both part of the essential nature of happiness.

My own view is that practical activity is somehow fundamental—though by this I do not mean to imply that public service for the benefit of others is necessarily fundamental (see Galston 1990, 87). Nor do I at all mean that one must actually win an election and hold public office in order to actualize one's full potential as a human being. Rather, the effectual truth (to borrow a phrase from Machiavelli) of the notion that theoretical perfection requires the transcendence of the body is that one must write (Walzer 1985, 261). Writing is a practical activity in that it both aims at the author's own happiness and is a creative activity. I can think of no compelling reason why legislative activity (writing the laws of a people) or, at any rate, preparing the ground for such legislative activity should not be included in the kind of writing that is essential to the philosopher's practical activity. Nor do I see this practical activity as ultimately incompatible with theoretical perfection. On the contrary, if we know only what we make, then some kind of creative activity is essential to theoretical activity.

I am aware that for many scholars, the notion that we know only what we make is a distinctively modern notion, certainly not one to be found in Plato or Alfarabi. For reasons that go well beyond the scope of this paper, I reject this view. Plato and Alfarabi (in contrast to Socrates, who wrote nothing) show by their deeds the importance of creativity to the highest human life. It is not necessary to separate knowing from making, theory from practice, in order to discover the difference between ancients and moderns. The distinction between ancients and moderns would be sufficiently established if it could be shown that only for the moderns does the knowability of what we make lead to the possibility of wisdom. Nowhere do Plato or Alfarabi reach the conclusion that through knowing (so far as we are able) the things that we have made, we may thereby hope to know everything, because we have in principle made everything. On the contrary, the admission that we know *only* what we have made is a recognition of human finitude. There is no necessity to follow Strauss in separating theory from practice, knowing from making, in order to avoid the hubris of trying to turn philosophy into wisdom (see Colmo 1990, 158).

THE JUST CITY

According to Strauss, Alfarabi clearly distinguishes between theory and practice, with theory ranking higher. The goal of the best way of life is essentially theoretical, not practical. Yet Strauss also insists that philosophy cannot be separated from self-knowledge or from moderation. Self-knowledge includes the realization of the need for the truth about the whole, as well as "of the difficulties obstructing its discovery and its communication" (1945, 366). Self-knowledge is essentially practical or political knowledge. Strauss seems to separate the theoretical and the practical while at the same time tying them together.

The contradiction in Strauss's position (at least within the Alfarabi essay) is merely superficial. With the problem of communication, which Strauss raises in this context, we are clearly in the realm of politics—not in the esoteric sense of the practical knowledge that leads to philosophy but in the ordinary sense of one's political relations with one's fellow human beings. The philosopher's self-knowledge is essentially practical or political knowledge in a sense that includes knowing how to get along with one's fellow citizens. The philosopher knows that his knowledge as a philosopher is strictly theoretical; for it is knowledge about the things that are independent of human choice or action, not about the objects of choice and action that concern the vast majority of the philosopher's fellow citizens. The philosopher is not an expert of the sort that knows how to get things done. Only the fellow citizen who is a potential philosopher might benefit from the philosopher's practical knowledge that philosophy is the best way of life. But this very knowledge might be harmful to most citizens, since it "is tantamount to closing the very prospect of happiness to the large majority of men" (1945, 378). Most human beings cannot be philosophers and are, hence, barred from the best way of life. But the philosopher must not communicate this knowledge, lest he drive his fellow citizens to anger or despair. For this reason, the philosopher's self-knowledge, in so far as it governs his relations with other citizens, is primarily a knowledge of the need for moderation.

Strauss does not go so far as to advocate, in the name of moderation, a philosophic withdrawal from political life, although he does mention this alternative (1945, 362). What he suggests is the abandonment of what he calls Socrates' "revolutionary" quest for the just city. It is not necessary for the philosopher openly to challenge the city, as Socrates did, thereby incurring, as Strauss puts it, "persecution and violent death" (p. 383), a phrase that seems to owe more to Hobbes than to Alfarabi. Alfarabi's Plato offers an alternative to the revolutionary way of Socrates:

> Plato substituted for [the revolutionary quest] a much more "conservative" way of action, *viz.*, the gradual replacement of the accepted opinions by the truth or an approximation to the truth. The replacement, however gradual, of the accepted opinions is of course a destruction of the accepted opinions. But being emphatically gradual, it is best described as an undermining of the accepted opinions. (ibid.)

In keeping with this "conservative" line of action, the philosopher–king who rules openly is replaced by the "secret kingship of the philosopher who lives privately as a member of an imperfect community" (p. 384). Although the philosopher lives privately, this does not necessarily mean that he altogether withdraws from politics. He will not openly confront the city, but he still may exercise a kind of "secret kingship"—through his writings, for example.

Strauss and Alfarabi are in full agreement that any political influence exercised by this private or secret rule will be "emphatically gradual," but it is still necessary to ask what the goal is of this gradual philosophic political action. Strauss answers,

> The goal of the gradual destruction of the accepted opinions is the truth, as far as the elite, the potential philosophers, is concerned, but only an approximation to the truth (or an imaginative representation of the truth) as far as the general run of men is concerned (1945, 384).

The aim of the philosopher's gradual political action with respect to the potential philosophers is to undermine—and ultimately to destroy—their belief in the accepted opinions of their community. But why does Strauss call this way of acting "conservative"? The fact that it is gradual and secretive merely makes it conspiratorial, not conservative. It is conservative, however, with respect to the accepted opinions of the vast majority of citizens. These opinions can be at best only approximations to the truth. This might suggest that the activity of liberating the philosophers is merely a more extreme version of the activity of approximating the truth for the majority of people. The two activities might seem to go hand in hand; but in fact, they are opposed to one another. No matter how close the accepted opinions might be brought to the truth, these opinions are false. For example, the political truth is that one should act morally because moral action is good for its own sake—or so says Strauss, though Galston reports that "the doctrine that actions must be chosen for their own sake to qualify as moral does not appear in Alfarabi's writings" even where one would expect it for moral and political reasons (1990, 172).[9] For the philosopher, however, morality is, in Strauss's words, "merely a means toward" the true happiness of the philosophic life (1945, 387). The potential philosopher must somehow be taught the philosophic truth about morality in such a way that teaching does not corrupt the nonphilosophic many. A parallel situation exists with respect to religion: "Conformity with the opinions of the religious community in which one is brought up is a necessary qualification for the future philosopher" (p. 383)—even though an equally necessary qualification is that these opinions not retain their grip on the mind. Certainly, no philosopher would expose the religious errors of his community in an irresponsible attempt to create a secular society in which error does not exist. Every philosopher recognizes the need for moral and religious belief in a healthy political community. He also recognizes that these beliefs can never be converted into rational knowledge. The philosopher, as secret king, will do his best publicly to promote the accepted opinions with respect to morality and religion while esoterically working through his books to destroy the hold of those opinions over the minds of potential philosophers. This is the goal of the gradual, secret political activity of the philosopher as understood by Strauss.

The political activity of the philosophers aims at the benefit of potential philosophers, but what of the philosopher's duty to the community as a whole? Certainly, the philosopher tries to avoid hurting the nonphilosophers through any action that would undermine the moral and political opinions of the community; but does the philosopher have a desire or an obligation to benefit the community as a whole in a positive way? Reforming the tax structure or improving the school system certainly are not the duty of the philosopher as philosopher; nor are they goals that are amenable to the secret, gradual activity of the philosopher. Strauss is altogether silent about this kind of political reform. Gradual improvement of the tax structure or of the school system are certainly possible; but they are not the business of the philosopher, and it is not likely that they could be brought about through esoteric writing (unless we are to interpret the Platonic dialogues as a massive contribution to school reform). Such reforms require just the kind of open, public participation in political affairs that Strauss's philosopher eschews. Gradual reform of the moral and religious beliefs of his community might be more in the philosopher's line of work, but this could only be done for the ultimate benefit of the potential philosopher. Altering the religious beliefs held by the vast majority of citizens can only mean the exchange of one darkness for another, for these beliefs can never be converted into rational knowledge.

In the light of this philosophic conservatism with respect to political action, how are we to understand Plato's claim that the just city comes into being through the rule of the philosopher (*Republic* 473d–e)? Alfarabi tells us that the rule of the philosopher is not possible in imperfect cities (1969, 62), and Strauss adds that imperfect cities constitute "the world as it actually is and as it always will be" (1945, 381). In other words, "there are examples of men of the highest excellence whereas there are no examples of cities of the highest excellence" (1964, 49). Hence, the philosopher must learn to live in the nonphilosophic city, a place in which he is always "in grave danger" (Alfarabi 1969, 67; Strauss 1945, 382). Part of the philosopher's self-knowledge involves learning to cope with this danger. He must learn to "adjust himself to the requirements of political life, or to the ways and opinions of the vulgar" (1945, 383). To some extent, then, the philosopher must, after all, assimilate himself to the ways of the vulgar (see p. 362). The philosopher's self-knowledge is moderation. Precisely because the theoretical way of life can never be translated directly into moral or political practice, it is necessary that theory not be separated from practical wisdom (though Strauss does not say whether philosophy and moderation can "coexist in their plenitude"). Strauss's moderate conclusion is that there can be no revolution leading to the just city.

I shall try to summarize Strauss's interpretation of Alfarabi on the subject of the political activity of the philosopher. The philosopher will not confront the nonphilosophic community in which he lives. Indeed, because he is a philosopher, he is in danger from the moral and believing community. He may consider himself lucky just to be left alone. Moreover, his greatest pleasure and happiness is not political activity of any kind but, rather, theoretical contemplation. For these reasons, the complete withdrawal from political life must be tempting. If he does not succumb to this temptation, it can only be because of his interest in and care for those in the community who are potential philosophers (but see Galston 1990, 87). His attempt to communicate with these future philosophers poses a grave threat to the moral and religious beliefs of the community. Hence, the philosopher must undertake to communicate the most daring or radical thoughts in a way that minimizes their political impact.[10] In this respect, the political action of the philosopher can correctly be described as conservative.

It is probably not possible in this case to repeat my approach of contrasting these sentiments with anything Strauss said later. He did not change his views.

It is, however, instructive to compare Strauss's statement with the last page of Alfarabi's *Plato*, upon which he bases it. Alfarabi says that Plato

> mentioned the Athenians (his own people) and their ways of life. He described how to abolish their laws and how to turn them away from them. He described his view regarding the way in which they could be moved gradually, and he described the opinions and the laws toward which they should be moved after the abolition of their ways of life and laws. (1969, 67)

Nothing could be more striking than the contrast between Strauss's "conservative" way of action (which is not to be confused with a conservative way of thinking) and the revolutionary tone of Alfarabi's closing paragraph.[11] Strauss and Alfarabi agree that change must be gradual, but Alfarabi mentions as the ultimate goal much more sweeping changes than anything even hinted at by Strauss. Indeed, Strauss does what he can to soften Alfarabi's language. For example, while Alfarabi refers, in the full passage, three times to the destruction or abolition of the generally accepted ways of life and corrupt laws, Strauss says that this destruction is "best described as an undermining" (1945, 383). The best description, it would seem, was not Alfarabi's. Strauss's wording made it less shocking. Instead, Alfarabi goes out of his way, as it were, to alarm the reader with the thought that at least one philosopher, Plato, sought to abolish the way of life of his own people. Moreover, Plato sought to act as a legislator, replacing the old laws with new laws.

Strauss's esoteric reading of the passage just quoted tends to make it more conservative, more moderate, than it appears to be on the surface. By Strauss's own account of esoteric writing, however, one would expect the surface of a statement to be more moderate than the hidden truth. At least one detail of the passage suggests that Alfarabi's true intention is more radical than what he says on the surface. We notice that Alfarabi explains that the Athenians are Plato's own people. Is it possible to conceive of a student of the *Plato* who did not know this? And if this knowledge is helpful, why withhold it until the end of the book? Strauss is surely right in thinking that the *Plato* is an esoteric work; it teaches by implication and insinuation. Under the circumstances, the notion that a philosopher might attempt to destroy or abolish the way of life and the laws of his own city or nation can only be taken as a subtle (but not too subtle) challenge by Alfarabi to his own people and to the Islamic law, the *shari'a* (1945, 372, n. 40). Naturally, in relation to Plato, Alfarabi speaks of changing the Athenian *nomoi* (laws); but he also uses the comprehensive expression "ways of life," which in the case of his own people would surely extend to the *shari'a*.[12] I do not think that Strauss's overall interpretation of the *Plato* allows him to notice the possibility that Alfarabi may here be making an amazingly bold declaration of his own political purpose (see Mahdi 1973, 25, n. 10). Strauss certainly does not mention this point.

As we have seen, Strauss does notice and make use of a statement in the *Attainment of Happiness* in which Alfarabi counsels that "conformity with the opinions of the religious community . . . is a necessary qualification for the future philosopher" (Strauss 1945, 383–84 and 373, n. 41). The philosopher accepts the world as he finds it; and this, certainly, is in some sense true. Strauss supports his "conservative" interpretation of Alfarabi by citing a reference in the *Attainment of Happiness* to outward conformity in matters of religion. Strauss interprets the last page of the *Plato* in the light of a statement made in the *Attainment of Happiness* and uses that statement to make the conclusion of the *Plato* seem more moderate or conventional than it might otherwise seem. This is surprising, to say the least, given the "canon of interpretation" that Strauss wishes the reader "scrupulously to follow": "Apart from purely philologic and other preliminary considerations, one is not entitled to interpret the *Plato*, or any part or passage of it, by having recourse to Farabi's other writings. One is not entitled to interpret the *Plato* in the light of doctrines, expounded by Farabi elsewhere, which are not mentioned in the *Plato*" (p. 375). Strauss explicitly mentions that one must favor the *Plato* in any place where its teaching conflicts with the *Attainment of Happiness*. Moreover, it is clear that Strauss particularly has in mind the priority of the *Plato* on religious matters. He is aware of the "deep silence" of the *Plato* with respect to the other life, the immortality of the soul, the soul itself, and even the Platonic ideas and the *nous* (intellect) by which the ideas are known (pp. 364, 371–72).[13] He is aware—and he has made us aware—that by this silence Alfarabi rejects these ideas or beliefs. But the *Plato* observes a deep silence (or, at any rate, a direct or explicit silence) on the issue of religious conformity. It is hard to avoid the conclusion that by introducing the idea of the necessity of religious conformity into his interpretation of the *Plato*, Strauss does not pursue the implications of his own canon of interpretation.[14]

What are those implications? How will we understand the last page of the *Plato* if we do not read it in the light of pious statements that Alfarabi makes elsewhere? Certainly, the *Plato* does not deny that the philosopher must be circumspect; he is, after all, in grave danger. Indeed, Alfarabi makes it clear that the art of Thrasymachus (i.e., the art of the sophist) may be most useful to the philosopher in speaking to the multitude (1969, 66; Strauss 1945, 383). Alfarabi translates the title of the Platonic dialogue *Sophist* as "falsifier." One of the most necessary aspects of such falsification in Alfarabi's circumstances must surely have been outward conformity in matters of religion.[15]

Even so, it is clear that the *Plato* is much more reticent than the *Attainment of Happiness* in stating the need for conformity in religious matters. I do not believe that this difference of emphasis can be understood on the basis of Strauss's interpretation of the *Plato*. Strauss is primarily concerned, in "Farabi's *Plato*," with the fact that most people "are eternally barred, by the nature of things," from philosophy (1945, 381). Hence, if philosophy is to be possible at all, then it must be possible in imperfect, nonphilosophic cities. According to Strauss, Alfarabi's "last word" on this subject is that philosophy and the perfection of philosophy do not require the establishment of the perfect political community (ibid.). Consequently, Strauss turns the focus of his attention away from the question of how to improve political life and toward the question of how the philosopher can survive and continue to philosophize in imperfect cities taken as they are. In trying to deal with this problem, Strauss pursues his inclination or tendency to separate the philosophic, or theoretical, from the practical, or political. Indeed, separation becomes opposition as philosophic radicalism is juxtaposed with political conservatism. Turning to Alfarabi, we see that he also recognizes the severe limits of political change (see, e.g., p. 384, n. 69). He recognizes that the philosopher will never be glorified or exalted by the citizens (i.e., will never rule openly or officially) and that all change must be gradual (1969, 67). Strauss's interpretation notwithstanding, however, the *Plato* emphasizes the political change that is possible.[16] Outward conformity is not always necessary, at least not to the same degree or to the same kinds of laws. If anything, the *Plato* makes clear that Alfarabi may have overestimated, in a particular case, his ability to abolish the ways of life and the laws of "his own people."

Strauss suggests that when Alfarabi talks about the legislator who will bring into being the just city of the philosopher, Alfarabi means, by *legislator* a prophet, "the founder of a revealed religion" (Strauss 1945, 380; see Alfarabi 1969, 66). On this interpretation, the just city would not be possible except on the basis of revelation. Strauss leaves no doubt that since the just city is possible only on this basis, it is not possible as an actual city, or the just city is possible only "in speech" (1945, 379).

My own suggestion is that Alfarabi himself is an example of the kind of legislator he has in mind as the founder of what he always calls "the other city"—that is, other than the cities existing in his time but certainly not otherworldly.[17] If this makes Alfarabi a prophet—a subject nowhere explicitly mentioned in the *Plato* (but see Alfarabi 1969, 61)—then so be it. Alfarabi's "prophecy," or legislation, is clearly not based on divine revelation. The only plausible source of Alfarabi's legislative authority is his philosophic insight. Alfarabi's efforts to be the founder of "the other city" presuppose less opposition and more cooperation between the theoretical life and the practical, political life than Strauss seems to grant.

Notes

1. Published works dealing with Alfarabi span the period from Strauss 1935 to Strauss 1957.

2. I have used the English translation of all three works by Mahdi (1969). Unless otherwise indicated, all parenthetical citations in the text are to pages of Mahdi's translation of Alfarabi *with the exception* of references between 357 and 393, which refer to pages in Strauss 1945.

3. See, e.g., Strauss 1965, 29–30. I have no explanation for Strauss's statement, in the last paragraph of "Farabi's *Plato*," that philosophy is animated by a "conviction" about the life worth living—a word that seems to imply belief and will, rather than knowledge.

4. Meier correctly represents Strauss's theoretical emphasis when he quotes Strauss's reference to "the necessarily anonymous truth" (Meier 1988b, 765; cf. Strauss 1945, 377). On Farabi's view of the "anonymous truth," contrast the relatively lengthy account of Protagoras' view (i.e., that the knowledge natural to man is not anonymous but relative to each individual) with the brief and obscure account of Plato's rejection of Protagoras' view (Alfarabi 1969, 54). This is one of only two places in the *Plato* (not counting the title) where Plato is mentioned by name. In the place where the anonymity of thinking is the issue, Alfarabi does not allow the philosopher to remain anonymous.

I am reminded that the Socratic ideas make the human good anonymous. It is indeed strange that Socrates and Descartes should share in their thinking the common feature that what is truly intelligible (the ideas in one case, body as extension in the other) is not alive. It is not surprising that a rift between the intelligible and the good has become a problem (*the* problem?) in Western thought. The mature Strauss points out a similar or related "defect" in the mature Socrates (1966, 59). In my view, one would expect Strauss to share this "defect" and hence not to notice it in Socrates. Obviously, my net does not catch all the fish in Strauss's waters.

5. These two alternatives are, in a way, present within "Farabi's *Plato*." Early in that essay, realization of the need to adjust to the requirements of political life belongs to "self-knowledge" (Strauss 1945, 366), while, later, that realization seems to derive from "insight into the nature of beings" (p. 383). The latter view seems very close to the position Strauss takes in "Philosophy as Rigorous Science and Political Philosophy."

6. While she does not offer an interpretation of the passage just quoted, Galston cites with approval the passage in which Strauss says that philosophy and kingship are not the same art. But this observation is appended to a paragraph stating that "Alfarabi appears to maintain the more extreme thesis that [practical rational excellence and moral virtue] are constitutive parts of philosophy itself" (1990, 64).

7. Alfarabi's *Enumeration of the Sciences* confirms the thought that the highest royal craft is supplied by one faculty, not two. See Mahdi 1975, 131–37.

8. Alfarabi says that the practical arts are not adequate for obtaining the desired knowledge or the desired way of life (1969, 59). But the desired way of life is by definition the product of a practical art (p. 60). The first statement makes sense only if it is limited to the generally accepted practical arts or to the practical arts practiced by the multitude (p. 59).

9. See n. 13.

10. Strauss's own publications from the last decade of his life might be thought to exemplify this kind of writing.

11. The contrast between the end of the *Plato* and Maimonides' *Guide of the Perplexed* 2.17 is also striking. Maimonides presents himself as building "a great wall" around the Law to protect it, a project that certainly sounds conservative. Strauss tells us that he studies Alfarabi in order to have the necessary background for understanding Maimonides (1945, 357, 393). Is it not possible, however, that Strauss's understanding of Alfarabi is colored by reading Alfarabi under the influence of a knowledge of Maimonides' intention to protect and preserve the Jewish law? Does Maimonides in fact become the background for Strauss's interpretation of Alfarabi? However that may be, the contrast I am suggesting between Alfarabi and Maimonides need not be explained in terms of basic philosophic differences. It is one thing to attack the *shari'a* at a time of Muslim power; it would have been quite another thing to attack the halakha during the Diaspora. On Alfarabi and Maimonides, see Berman 1974, esp. 163.

12. On the distinction between *nomoi* and *shari'a,* see Maimonides' *Guide to the Perplexed* 2.40. Galston comments that there is no reason "to assume that Alfarabi must have viewed the conventions of his time as disparagingly as Socrates regarded the way of life of the Athenians," but she offers no Alfarabian text as support for this claim or for her own view that "many features of Islam were a vast improvement over Greek morality and opinions" (1990, 173).

13. As Strauss points out, Alfarabi is silent about immortality while mentioning the *Phaedo,* in which immortality is, of course, a major theme. Strauss concludes, rightly I think, that this silence amounts to a rejection of the doctrine of immortality. According to Alfarabi's summary, the *Phaedo* seems to be a book about whether a man should be willing to die for the sake of the practice of the true moral virtues, this alone being sufficient reason to sacrifice one's life if need be (1969, 63). I take it that there is a connection between Alfarabi's rejection of immortality and his refusal to give a clear affirmative answer to the moral question he raises here. Alfarabi and Kant seem to agree that if morality is to be treated as an end in itself and if human beings are to be expected to make sacrifices commensurate with such an end, then it is necessary to assume that the soul is immortal. Alfarabi does not agree, however, in making this last assumption.

14. One could avoid this conclusion if one assumed that Strauss is here writing esoterically. Given that Strauss is an esoteric writer, does that fact dispose of the present case? In this instance, we could avoid what seem to be contradictions in Strauss's argument by ignoring his "conservative" remarks about political action, just as we are inclined to ignore Alfarabi's admonitions to religious conformity or, at any rate, to minimize their significance. But the weight of the evidence is against this view of Strauss's intention in the case in point. For one thing, it is possible to discover in Alfarabi a political alternative to the acceptance of the imperfect communities existing in his time. Strauss's "conservative" remarks, however, seem to be his "last word" on the subject. I have placed the word *conservative* in quotation marks as a reminder that it applies only to Strauss's attitude toward political action; there is nothing conservative about Strauss's thinking.

15. This certainly does not mean that one compartmentalizes one's thinking so that one is a believer on holy days and a philosopher on other days (Strauss 1945, 374). See Alfarabi's criticism of the sophist Hippias, whom Alfarabi speaks of as if he were in fact two men (1969, 59). This passage helps us to understand how Alfarabi would distinguish between the philosopher and the sophist. The sophist is somehow a man in contradiction with himself. But then, we may ask, does anyone ever fully escape contradiction?

16. Muhsin Mahdi sees the realization of the political good as being Alfarabi's "central concern." Concerning the view that philosophers "should tend only their private gardens," Mahdi writes: "Perhaps there are times and places which necessitate these views. But one need not make a virtue out of necessity" (1981, 19–21).

17. According to Alfarabi, the title of the book dealing with the legislator (the *Epinomis*) means "investigator." In the *Plato,* the one who is repeatedly said to investigate is Plato himself. Alfarabi is, of course, also such an investigating legislator.

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FREEDOM, COMMUNITY AND FUNCTION: COMMUNITARIAN LESSONS OF MEDIEVAL POLITICAL THEORY

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The communitarian critique of liberal individualism is presently experiencing something of a decline. One of the reasons for the failure of communitarians to make a lasting impact on liberal theory may be the historical precedents to which communitarians have turned, such as Aristotle and civic republicanism. I argue that communitarian theory may more fruitfully draw upon a model of the relation between individual and community derived from the Latin Middle Ages. This approach—communal functionalism—claims that the community is essentially composed neither of individuals nor of citizens but, rather, of functional groups or parts arranged according to their contribution to the community. I consider two variants of this framework, John of Salisbury's "physiological" version and Marsiglio of Padua's "civic" account. I close with an evaluation of the relevance—and also some of the limitations—of the communal functionalist model in relation to the requirements of contemporary communitarian theory.

One of the most striking features of the debate between communitarians and liberal individualists that has dominated Anglo-American political theory during the last decade or more is the extent to which it has been conducted through the medium of the history of Western political thought. In particular, the communitarians, arguing for what has been called "the politics of the common good" (Sandel 1984; Taylor 1986), have often turned to traditional models of civic-based virtue.[1] Some have followed Aristotle's defense of a community which is composed of citizens who are competent both to rule and be ruled and in the public realm of which all are accorded an appropriate share of burdens and rewards (Beiner 1983; MacIntyre 1985; Nussbaum 1990). Others have looked to civic republicanism (as espoused by, e.g., Machiavelli, Rousseau, and Hegel) for its devotion to political participation as the expression of a uniquely civic mode of virtue (MacIntyre 1985, 236–38; Oldfield 1990a, 181–85; Taylor 1989, 165–72).

In all too many cases, however, such attempts to formulate communitarian principles along lines derived from earlier political theories have been faulty. Serious questions have been posed about the historical accuracy and philosophical fidelity of communitarian interpretations of Aristotle and the "civic republicans" (Mulgan 1990; Newell 1987; Oldfield 1990b; Yack 1985). Certain communitarian thinkers have merited the accusation of "romanticism" on the grounds that the earlier social practices and doctrines which they laud as built upon a substantive notion of the common good actually reflected a failure to appreciate alternative conceptions of that good and consequently mirrored the hegemomy of one set of racial, gender, or class interests (Gutmann 1985, 318–29; Holmes 1989, 239–40; Kymlicka 1990, 225–27).

Behind the complaints raised by liberal critics with regard to the communitarian appropriation of the past appears to be the assumption that classical Greek and republican notions of the common good were so thickly defined as to exclude a realm of individual, independent judgment about social (or personal) goods or virtues from emerging. While this seems to me to be essentially true (neither Aristotelian nor republican conceptions of the good permitting much room for a diversity of viewpoints about basic ends or definitions of a "way of life"), it does not justify the further conclusion that *all* traditional accounts of the public good are guilty of the same "thickness." Liberal theory seems to take largely for granted that every conception of the communal good that moves beyond the minimal (or thin) requirements of public order and tolerance falls prey to the same charge: it imposes an "exclusivist," quasi-totalitarian conception of a common life upon individuals, who are thereby prevented from choosing their own life plans. In sum, it is presumed that all versions of communitarianism are antiindividualistic and are so in the same way.

As a purely philosophical claim, this view is suspect; for the liberal understanding of the common good seems to trade on an ambiguity that has become attached to previous communitarian usages of that phrase. Communitarians have generally sought to argue that a community is defined in terms of a substantive good or set of goods that generate a fundamental civic identity within which all members are embedded and to which they ought to be socialized. Liberals have been prone to construe this as equivalent to the rejection of individual diversity in relation to human goods. But it is one matter to claim the unity of the common good and another to assert the impossibility of different interpretations of the *same* common good (or different ways of realizing that good) according to the range of roles or positions within society. Unquestionably, communitarianism is committed to the view that a single common good exists; it need not (logically speaking) also be committed to the view that this good will be understood or applied identically by all persons. Instead, it seems plausible to adopt the view that the realization of any common good is conditioned by circumstance, in the sense that different persons occupying divergent life situations will conceive of the common good in a manner relevant to their social needs and surroundings. This is not to say, however, that such persons adopt substantively differently or incompatible conceptions of that common good. It is rather to assert that their public selves are not formed abstractly or generally (by reference to a mode of civic virtue identical for all citizens) but are shaped in a concrete and mediated fashion by way of a determinate role or position within the overall communal structure. The communitarian supposition that there *is* a common good is thus upheld in a manner consonant with the existence of diverse points of view regarding that good.

Nor does this alternate version of the communitarian thesis lack historical precedent; for if one looks not to the ancient Greeks or the modern republicans but, instead, to the Latin Middle Ages (a period too often ignored entirely by political theorists) one discovers a theoretical framework, that I label *communal functionalism,* which deflects some of the more serious charges against communitarianism. At the heart of communal functionalism is the claim that the community is, in the first instance, composed neither of individuals nor of citizens but, rather, of functional groupings or parts, arranged according to the nature of their contribution to the communal whole. This is essentially an organic conception of social life but one with certain distinctive features; for it defines membership in the community as a direct result of one's contribution, through the performance of a given function, to the well-being or health of the whole. Yet although types of contribution to the community are distinguished—and specialization of function (or division of labor) thereby posited—there is no attempt to postulate a hierarchy of functions in order to exclude some part or parts from a place in public life or to cast aspersions on the qualifications of certain groups to exercise a basic role in government. On the contrary, any person from any segment of society that contributes in any way to the welfare of the communal unit is thereby accorded an equal capacity to gage the needs of the community and to perform one's functions accordingly. This means that the community must guarantee a basic liberty of judgment and expression for all its members, regardless of the tasks that they perform.

In contrast with the mode of specialization typical of Plato's ideal polis, the division of responsibilities according to the communal functionalist model entails the diffusion of concern about the good of the whole. Communal functionalism does not transform ruling into a specialized art, nor does it lead to a corporate pluralism that limits the participation of the various parts of society to promotion of their narrow self-interests within a public forum. Rather, it maintains that each segment of the community, precisely insofar as it contributes to the good of the whole in a particular fashion (by performing some specific function), also thereby acquires a general duty toward the maintenance of the social organism. There are no grounds upon which the exercise of this general duty may legitimately be suppressed; the maintenance of the common good, as opposed to rule in accordance with some private or partial set of interests, is deemed to depend upon the ability of each and every member to determine autonomously the standards by which the healthy community is to be governed.

The approach to the relation between the community and the individual afforded by communal functionalism seems to resolve many of the valid concerns voiced by liberal individualists about the sort of holism implicit within communitarianism, since it acknowledges the indispensability of individual rational judgment in regard to the public welfare. Claims of superior status or authority in themselves carry no preeminent weight in the process of public deliberation and decision making. Communal functionalism resists efforts to override or undercut personal choice. The lack of respect for individual intellect and volition of which communitarianism sometimes stands accused is not a factor. The terms of the model are violated by the denial to any segment of the community of the opportunity to dissent or to offer rational arguments about matters touching upon the interpretation of the public good.

Expression of the basic principles of communal functionalism abounded during the Middle Ages.[2] Generally speaking, however, the doctrine was articulated within either an organic or a civic framework. The first view is found in the mid-twelfth-century philosopher and churchman John of Salisbury, who concentrates upon the interrelation of parts constituting the "physiology" of the body politic. The second is expressed by Marsiglio of Padua, an early fourteenth-century scholastic philosopher and antipapal polemicist, who appeals to the formal consent of the constituent parts of the community. I shall examine both the diversity and unity present within communal functionalist discourse: communal functionalism is not a single, narrowly specified set of doctrines but a general matrix, a paradigm, within which much creative theoretical discussion occurred during the Middle Ages. I also seek to evaluate some of the lessons that communal functionalism may be capable of contributing to current communitarian debate, as well as to assess potential limitations within the framework.

COMMUNAL PHYSIOLOGY

Although the organic metaphor comparing the political community to a living creature was widely employed during the Middle Ages, it is generally agreed that John of Salisbury's version of the analogy, presented in his major treatise, the *Policraticus* (completed in 1159), was among the most innovative and influential (Dutton 1983; Struve 1978). John begins with the simple observation that the commonwealth may be likened to a "body which is animated" (1909, vol. 1, 282; idem 1990, 66).[3] The differentiation of the offices of political society may thus be represented in a manner analogous to the distinction of the parts of the human anatomy. Like all bodies, the commonwealth is guided by a soul; John asserts that the place of the soul belongs to "those who direct the practice of religion" (1909, vol. 1, 282; idem 1990, 67). Yet the clerical soul of the polity is not, strictly speaking, a member of the commonwealth, just as the eternal human soul is not coextensive with the mortal physical organism it directs. The political creature is an essentially secular entity. Thus, the body politic, properly speaking, is ruled by the prince, who "occupies the place of the head" (1909, vol. 1, 298; idem 1990, 69). At the heart of the commonwealth lies the senate, composed of the counselors whose wisdom the ruler consults. The senses correspond to the royal judges and local agents such as sheriffs and bailiffs who exercise jurisdiction in the king's name. The financial officers constitute the stomach and intestines of the body, while the two hands are formed by the tax collector and the soldier. Finally, John compares the feet to the artisans and peasants "who erect, sustain and move forward the mass of the whole body" (1909, vol. 1, 282–83; idem 1990, 67).[4] Each of the parts of the organism, according to the *Policraticus*, has its own definite function, which is fixed by its location within the overall scheme of the body. None of the members may be excluded or removed without serious damage to the whole.

John's functional depiction of the community was not, as is sometimes supposed, primarily intended to justify hierarchy and division within society. Rather, the body politic is invoked in the *Policraticus* as the expression of a principle of cooperative harmony through which otherwise disparate individuals and interests are reconciled and bound together (see my earlier study, Nederman 1987a). John adopts a "physiological" approach to the political organism in the sense that he emphasizes "organs cooperating by virtue of voluntaristic categories such as common purpose" (Jung 1985, 172). Throughout his writings, he regards "reciprocity" as the salient characteristic of natural and social systems (John of Salisbury 1929, 5–8; idem 1955, 181). The *Policraticus* insists that "there can be no faithful and firm coherence where there is not a tenacious unity of wills and a virtual joining together of souls themselves. If this is missing, men harmonize their works in vain, since deceit advances into catastrophe without an attitude of helpfulness" (1909, vol. 1, 308–9; idem 1990, 77). All the parts of the body, in other words, must be truly dedicated to a common or public welfare which supersedes the aggregate private goods within the polity. The ruler and magistrates are advised to attend "to the utility of all" (1909, vol. 2, 73; idem 1990, 136); the feet are counseled "to concentrate on the public utility in all matters" (1909, vol. 2, 59; idem 1990, 126); and in general, "all the members" are expected to "keep watch over the public advantage" (1909, vol. 2, 71; idem 1990, 135). The security of the body politic can only be maintained by means of a joint commitment to a public good that benefits every part without distinction, so that "each individual may be likened to a part of the others reciprocally and each believes what is to his own advantage to be determined by that which he recognizes to be most useful for others" (1909, vol. 2, 59; idem 1990, 126). John's political body is one in which, beyond all social differentiation, there is "mutual charity reigning everywhere" (1909, vol. 2, 86; idem 1990, 142), because all segments are attuned to the same enduring common purpose encompassing the valid interests of the whole. Unity follows from cooperation, and cooperation stems from the existence of a good shared by the entire community and each of its members.

What ensures the presence of a cooperative spirit within diverse sections of the community? How is fragmentation to be avoided? The answer stems from John's identification of the "health" of the body politic with the practice of justice by the organs and members. Indeed, his definition of the common good in terms of justice contains a recognition of a correlative obligation on the part of all members of the commonwealth: "Inasmuch as the duties [*officia*] of each individual are practised so that provision is made for the corporate community, as long as justice is practiced, the ends of all are imbued with the sweetness of honey" (1909, vol. 2, 63; idem 1990, 131). Every organ of the body must conduct itself according to the dictates of justice if the polity is to exist as a corporate whole. This is implicit in John's conception of justice. Following Cicero's formula in *De officiis* (1913, 25), the *Policraticus* asserts: "Justice consists chiefly in this: do not do harm and prevent the doing of harm out of duty to humanity. When you do harm, you assent to injury. When you do not impede the doing of harm, you are a servant of injustice" (1909, vol. 1, 277; idem 1990, 62). The essence of justice pertains to a responsibility toward others; this responsibility is not simply constituted by a negative obligation to refrain from the commission of injury but entails a positive duty to protect others from harm as well. Justice is inherently productive of social cooperation, whereas injustice necessarily tends to human disharmony and social disintegration. Thus, John believes that justice is a generalized virtue; for since justice is the salient characteristic of the common good, it determines how each of the bodily members performs its functions. Not only kings and magistrates but even the lowliest of parts are viewed by John as potential agents of public justice (see Nederman 1987a, 220–21). This is established by the very fact that the function of each segment of the organism is absolutely necessary for the well-being of all the others.

BODY AND INDIVIDUAL

Supporting John's argument for the wide diffusion of responsibility for the maintenance of justice is the view that all members of the body politic possess the basic faculties necessary for guarding and protecting the common good. John upholds the ability of each part of the community to discover for itself what is beneficial and just and to act thereupon. This principle accords with the general terms of his account of moral decision making. The *Policraticus* claims that "discretion with regard to place, time, manner, person and cause . . . most readily separates out" virtue from vice (1909, vol. 2, 316; idem 1990, 187). In deciding how to conduct oneself, each individual must consider all relevant conditions and choose the course of action which is appropriate in its context. Thus, John believes that moral reasoning is ultimately a matter of individual calculation and determination (Nederman 1986, 133–37). To do what is just, one must apply one's general knowledge of justice to the particulars of the case and decide upon the correct action. As applied to the political community, John's prescription of moral autonomy means that all organs and limbs (rather than simply a specialized few) enjoy the capacity to engage independently in the process of moral reasoning about matters of justice.

In turn, independence of moral judgment depends on personal freedom of thought and expression. The *Policraticus* claims that the very precondition for rational decision making is personal liberty. Consequently, John posits an intimate connection between virtue and liberty. He remarks that "virtue does not arise in its perfection without liberty and that the loss of liberty demonstrates irrefutably that virtue is not present and therefore that anyone is free according to the virtue of his dispositions" (1909, vol. 2, 218; idem 1990, 176).

Specifically, the relation of liberty to virtue stems from the need for individuals to be free to make their own circumstantial determinations about morally good actions. Liberty thus, according to John, consists in the careful formulation of sound rational judgments about the most appropriate route to virtue: "Liberty, therefore, judges in accordance with the free will of the individual" (1909, vol. 2, 217; idem 1990, 175). As applied to matters of justice, this doctrine teaches that only people able to make free use of their rational faculties can perform their duties toward the body politic; and since the health of the organism requires that every limb and organ must share full membership in the body, the free judgment of each segment of the community must be encouraged and defended. Hence, one of the prime functions of the good prince is the defense of the liberty of his subjects; and the tyrant is characterized as the ruler who enslaves the other parts of the community by denying to them their independence of judgment (1909, vol. 2, 345; idem 1990, 191).

John takes this freedom of judgment quite seriously. He argues that liberty of expression and action is only meaningful when the opportunity exists to make it effective. Therefore, he encourages virtuous men to denounce the wrongs and injuries committed by others: "Man is to be free and it is always permitted to a free man to speak to persons about restraining their vices" (1909, vol. 2, 224–25; idem 1990, 180). Likewise, he urges wise men to heed patiently the criticisms offered by their fellows (1909, vol. 2, 219, 223; idem 1990, 177, 179). Liberty of judgment is to be extended without regard for rank: the *Policraticus* praises rulers and great men who allow social inferiors to vocalize condemnations of the vices of their superiors (1909, vol. 2, 219–23; idem 1990, 177–80). John even recounts his own highly censorious comments to Pope Adrian IV about the evil conduct of the Church and the papal curia (1909, vol. 2, 67–71; idem 1990, 132–35). In sum, John ties the good order of the community to the personal freedom of its members. In his view, the protection of liberty of thought safeguards against the rise of tyrants and enemies of the common good.

John is willing, at least under exceptional circumstances, to extend this individual discretion into the realm of direct political action against evil rulers. He distinguishes between two different circumstances that warrant different types of public concern, namely, the commission by rulers of minor offenses (which can be tolerated by the community) and flagrant crimes, which endanger the well-being of the body politic (1909, vol. 2, 78–79; idem 1990, 140–41). In the first instance, citizens should freely express their disapproval and recommend corrective measures but should not challenge the position of the governor. In the second case, when the common good is threatened by a tyrant, members of the political organism ought—indeed, are bound by the functionalist terms of their inclusion within the political order—to take it upon themselves to deflect injury to the public welfare. This claim forms the basis for John's famous doctrine of tyrannicide (Nederman 1988). The *Policraticus* regards tyranny to be a crime against "the body of justice" (*corpus justitiae*). Consequently, inasmuch as one enjoys membership within the political community, one has an ultimate responsibility to eradicate the incorrigible tyrant. John asserts that "whoever does not prosecute [the tyrant] transgresses against himself and against the whole body of the secular republic" (1909, vol. 1, 233; idem 1990, 25). No overt limits are set on the social position or station of the tyrant slayer. Rather, because action against a tyrant stems from the general duties grounded in one's functional position within the community, any of the members is equally authorized to act. The example of a duty to kill the tyrant may be the most extreme case of a participatory duty that nonetheless requires individual freedom of judgment; but it illustrates the strength of the link between personal liberty and the active involvement of each and every member of the body in defending the good of the political organism.

That John seeks to allow for meaningful individual choice in the context of an argument for a high degree of political cooperation is perhaps the most striking feature of his argument. This is rooted firmly in his communal functionalist outlook. Inasmuch as everyone has a task within the body politic, each must be free to act as is seen fit for the sake of—or at least in a manner commensurate with—the common good.

Likewise, one is prohibited from acting against the public welfare. Liberal individualists should derive some satisfaction from this approach because it permits each person to deliberate autonomously about how the common good is best to be served and leaves it to each to act accordingly; yet this approach vindicates the central tenet of the communitarian cause by insisting that members of the body politic have a duty (founded on the reciprocal benefits that they and the community receive from each other) to exercise their discretion with regard to the utility of the whole. Members are not free to abdicate their responsibility to judge for themselves. Indeed, the abdication of such autonomy is for John the sign of a servile populace, one incapable of virtuous conduct (which must be freely chosen) and ripe for submission to a tyrant. John shares with liberal thought a strong distaste for tyranny but views tyrannical government itself as often the product of tolerance for withdrawal from the public sphere and refusal to consider the common good. On John's physiological account, such repudiation of responsibility toward the community is not merely self-regarding. It means that the functions one performs may no longer be counted on to augment the welfare of the rest of the community. It is exactly in this sense that the primacy of function serves to mediate between individual and community: it fixes both the legitimate limits of the public authority over personal choice and the nature of the contribution that the parts of the body may rightfully expect of one another.

A COMMUNITY OF PARTS

It should not disturb us too greatly that John of Salisbury does not employ a more overtly civic language in his account of the body politic and its parts. The vocabulary of citizenship was not so familiar to him as it would be to thinkers in later centuries. It seems clear that the features of membership in the organic community as described by the *Policraticus* are largely coextensive with a conception of civic identity. More disturbing, perhaps, is the absence from John's theory of any formal or regularized institutional mechanisms for the expression and discussion of individual judgments in regard to the common good. He takes for granted an entirely personalized framework within which individual discretion translates solely into individual action. Collective decision making, based on rational discourse or persuasion, does not enter directly into his scheme.

By contrast, the version of communal functionalism advocated by Marsiglio of Padua in his major political work, the *Defensor pacis* (completed in 1324), is not susceptible to the same criticism. With the benefit of more thorough knowledge of ancient political ideas and practices, as well as direct experience with the communal governments of his native Italy, Marsiglio presents a theory oriented more self-consciously toward the concepts and language associated with the civic body (*civitas*); yet Marsiglio shares certain fundamental premises about the composition of the community with John of Salisbury. In particular, Marsiglio's *Defensor pacis* gages the level of the "development" or "perfection" of a community according to the extent of the differentiation of its parts, where such parts are understood to be the specialized functional divisions within society. It describes a gradual process through which human beings, acquiring experience and improving their reason, discover all the tasks necessary for a satisfactory existence (1927, 10–11). Once this is achieved, it is possible to speak of the "perfect community, called the civic body, with the differentiation of its parts" (p. 11). The variegation of functions typifies the mature community in comparison with more primitive forms of association.

Marsiglio thus places a premium on reciprocity. The intercommunication of functions should be the goal of the multiplicity of parts: "Since diverse things, which cannot be procured by men of one order or office, are necessary to those who desire to live sufficiently, there needed to be diverse human orders or offices within this association, exercising or procuring such diverse things which humans require for sufficient life. And these diverse human orders or offices are none other than the many and distinct parts of the civic body" (1927, 14). The final cause of each member is the well-being of the whole community, rather than simply its narrow self-interest (p. 25). Marsiglio enumerates (in imitation of Aristotle's division in *Politics* 7) the six basic segments of the perfected community, namely, the agricultural, artisan, military, commercial, religious, and judicial (or ruling) parts (p. 15). Every one of these functions is revealed to be indispensable for the sufficient communal life which all human beings naturally desire (pp. 15–20). The parts are consequently regarded as parts *of the civic body*: they "are properly called offices [*officia*], as though services, since, considering that they are established in the civic body, they are ordered toward human service" (p. 27). These parts do not perform their functions in isolation but, rather, in the context of, and in relation to, the other elements of the civil community (pp. 8, 100–101).

Therefore, Marsiglio declines to draw the Aristotelian conclusion that the performance of certain types of functions (i.e., the manual tasks associated with farming, the crafts, and commerce) disqualifies persons from membership and participation within the civil order (Aristotle *Politics* 1328a; cf. Marsiglio of Padua 1927, 15). Instead, he infers from the need for diversity within the community that all the parts must be accorded a basic civic identity: this is the only way to avoid the disordering of the communal whole. The status of citizen encompasses "mechanics," craftsmen, and artisans just as surely as it does the leisured and educated elites (Marsiglio of Padua 1979, 182; see also Black 1984, 92–93).[5] The salient criterion for citizenship is one's contribution to the community judged along functional lines.

In order to ensure the smooth and harmonious operation of the various parts, it is necessary to establish laws and to appoint rulers who will enforce the standards which have been legislated; for otherwise, should conflict occur among the parts of society, there would be no fixed procedure for resolving it, and the community would dissolve into quarreling factions (Marsiglio of Padua 1927, 13, 73–74). Such laws and their enforcement must be entirely conducive to the common benefit lest some segment of the communal whole regard itself to be slighted or oppressed and withdraw its contributions. This means that every effort must be made to guarantee that public decision making is not inclined to favor or advantage one part at the expense of others (pp. 53, 58). In Marsiglio's view, the appropriate means of assuring this equity (and thus of harmonizing the disparate interests within the community) is the active participation of all citizens in the selection of rulers and the authorization of laws. Of course, the *Defensor pacis* does not deny that some parts of the civic body are better prepared to perform certain public tasks. For instance, he leaves the framing of laws to those whom he terms *prudentes*, that is, wise men of leisure (pp. 48–49); yet he explicitly rejects arguments that such men are competent to impose laws upon or govern the community in the absence of the universal assent of the citizens.

COMMUNAL CONSENT

The exercise of consent forms the very heart of the Marsiglian conception of citizenship (Nederman 1990, 1991). The community must be the final and binding determinant of its own welfare; thus, it must ultimately approve all laws by which it is governed, as well as the rulers who enforce those statutes. When the body of citizens enacts a law communally, through the consent of every (or virtually every) citizen, there is no question about whether the legislation expresses and accords with the public welfare. Marsiglio's theory of consent leads him to deny the claim that the vast mass of the citizen body is bound to be utterly vicious and incapable of discovering its own best interests. On the contrary, the *Defensor pacis* insists that "the entire assembled multitude can discern and will the common justice and benefit with greater prudence than any part taken separately" (1927, 58–59). There are no grounds for circumventing the mechanisms of consent: all decisions about public affairs, such as the implementation of new laws, must be placed before the whole citizen body in order to receive authorization. While acknowledging the usefulness of the law-framing function of the *prudentes*, Marsiglio regards this skill as a specialized one and therefore not comparable to the general capacity of each citizen to review proposed legislation for himself (p. 60). The citizen's place in the legislative process is comparable to the audience's position in relation to a work of art or technical endeavor. While only a few enjoy the creative or intellectual prowess and skill required to paint a canvass or design and build a house, it is within the grasp of almost every person to appreciate and judge the final product (p. 56). Although most individuals possess neither the leisure nor the education to discover and frame legal standards, Marsiglio believes that they "nevertheless participate in the understanding and judgment of practical matters" (p. 58). To deny such a full rational capacity to citizens would be to subscribe to the view (denounced by Marsiglio) that some segment or group enjoys privileged access to knowledge of the common good and thus is competent to rule and legislate without general consent.

The exercise of this basic power of reason must be matched by the presence of personal liberty if the consent of the civic community is to be effective. For consent itself assumes that the citizen is completely free to judge for himself the potential defects or inadequacies of proposed laws or magistrates. If a citizen is denied such liberty to formulate his own opinions and to express them, then genuine consent will not occur and statutes will be enforced that do not reflect the actual determinations of the community. The *Defensor pacis* proclaims: "Every citizen must be free and not endure the despotism of another, that is, servile lordship. This would not happen if some one citizen or a few of them made the laws according to their own authority above the corporation of citizens" (1927, 52). In the absence of consent, the governance of the polity would be reduced to the domination of some partial segment of the community and the freedom of citizens would be denied. Indeed, civic status disintegrates in the absence of freedom, given that citizenship itself for Marsiglio is constituted primarily by the active exercise of the public duty of consent. Each citizen is the necessary bearer of a capacity for autonomous rational judgment that must be freely exercised if the community is to be properly and peacefully ordered.

At the same time, the liberty underlying civic assent is not purely arbitrary: when approving law, citizens must consult not their own private wills but only the good of the communal body. Here, the duty of citizens becomes more obvious in Marsiglio's account. He views the original creation of civic institutions as a cooperative process in which humans came together in express recognition of their own naturally sociable propensities (1927, 353). Because political society was created by such consent, its members must perpetually accede to the terms of their own continued association (including their laws and rulers) in a sort of recapitulation of their initial entry into society (Nederman 1991, 32). Thus, the duty to engage actively in the consenting process stands at the heart of the existence and perpetuation of civil institutions and relations. Should citizens permit their private wishes to take precedence over the needs of the community, the very fabric of the political order itself would begin to come unraveled. Indeed, a "citizen" who prefers private aims to the public welfare ceases to be a citizen at all, by refusing to acknowledge that basic duty toward the common good that is the defining quality of citizenship.

Hence, just as in the organic account of John of Salisbury, Marsiglio posits a notion of liberty that is inalienable yet communally grounded. No one may deny freedom of judgment to the individual citizen. To do so would be to harm the well-being of the community by excluding some of the contributions that are constitutive of its perfection. At the same time, however, the liberty that rests on function is not essentially private or self-regarding. The exercise of free choice, instead, reflects public needs and goods, so that citizens are not free to remove themselves from the political realm while still enjoying the other advantages of civil life. In a sense, there would be no liberty if there were no community; but the reverse is also true.[6] Thus, personal freedom of judgment and communal duty are both complementary and mutually reinforcing in Marsiglio's thought. They receive a formalized and institutionalized expression in the process of civic consent to laws and rulers. This seems to be the crux of the divergence between a Salisburian and a Marsiglian account of the functionally based community. Where John leaves the decision-making process entirely up to the individual's reflection and discretion, Marsiglio permits—indeed, encourages—interaction and discussion as a prelude to consent. The members of John's polity are essentially moral agents whose scope of concern is personal virtue as it impinges on the body politic. Marsiglio's parts are more fully citizens in the sense that they assemble to speak and listen to one another prior to determining matters of public welfare.

LESSONS AND LIMITATIONS

In their careful attention to the functional bases of human diversity, both John and Marsiglio are attuned to the important implications of difference for the life of the community. They realize how easily the distinct elements of the communal whole can lose sight of the common good that binds them and begin to concentrate upon the realization of purely private goods, which inevitably has a disintegrative effect on civic identity. On the one hand, their essential approach admits of the permanence of diverse experiences and orientations. Distinctions of function (coextensive with divisions of wealth, status, leisure, etc.) are deemed to be fixed features of human existence, underlying all "ways of life" that a community might seek. Those who look to the community to break down the division of labor (or the social consequences of that division) will not find the communal functionalist framework rewarding. On the other hand, by starting with the concrete basis for the life experiences of most people, John and Marsiglio are better prepared than many communitarians to address the real foundations of divergence between differing perceptions of interest (common or private) within a community. After all, "the citizen" is as capable of being rendered an abstraction as "the individual." By occasionally overlooking the extent to which the boundaries of the self are set by functional circumstances (modes of employment, education, status and culture), communitarians readily open themselves to the charge of formulating "civic virtue" or "citizenship" as a universal category standing outside the historical and empirical situation of individuals within specific civic bodies.

Communal functionalism resists this tendency to abstraction by taking as its point of departure the tasks and activities which make possible a given society at its particular level of development. Citizenship is rooted in what individuals *do*, rather than what they are. A person is a citizen by virtue of the performance of certain functions that contribute to the welfare of the whole. (A person is a *good* citizen when these functions are performed in accordance with the needs of the community and when concern is evinced for the communal good by way of other sorts of public behaviors.) The duties of citizens are consequently not static, but are contextualized according to the immediate requirements of the public utility as recognized and accepted by their own free judgment. The community (and therefore its good) is not a simple unity but is, instead, a multiplicity ordered toward a single end—as Marsiglio, indeed, tries to explain (1927, 94–96). Without surrendering a doctrine of substantive common good, communal functionalism permits the communitarian to construct a framework within which to understand the diversity of interpretations of that good and to appreciate the necessity of the diversity itself.[7]

Thus, a robust theory of participation is implied by communal functionalism; for since every distinct function within society generates a unique and valuable point of view regarding the common good, there must be some mechanism for the expression and harmonization of these visions—in essence, an outlet for sharing perspectives and appreciating the totality. Communal functionalism counsels tolerance for divergent interpretations but also an openness to the realization of one's interdependence in relation to other segments of the community. The participatory process ensures some exchange of alternate visions in a fruitful manner, in the sense that it will encourage a cooperative consensus, to which the viewpoint of every part or member contributes. Such participation may be informal (the freedom of speech and criticism proposed by John of Salisbury) or rigorous (Marsiglio's insistence on individualized consent); but it cannot be foresworn. The liberty of judgment founded on the diversity of functions relating to the common good necessitates, for both John and Marsiglio (and surely for all accounts of communal functionalism), some effective avenue for its communication and realization.

It follows that communal functionalist theory simultaneously strengthens the concepts of the individual and the community. This may appear self-contradictory, in light of our automatic tendency to view any reinforcement of the status of the individual as a challenge to the community and any bolstering of communal values as an infringement on the individual; but for the medieval world, these two factors were not mutually exclusive. We have seen that John and Marsiglio conceived of the governance of the well-ordered community as subject to evaluations of goodness, knowledge of which was accessible to every member of the community in virtue of one's function therein. Consequently, it followed for them that the more freedom enjoyed by individuals and the more they were allowed to employ their powers of reason, the stronger would be their commitment to the communal good and to the duties entailed thereby. In other words, John and Marsiglio were satisfied that free individuals, left to the devices of their own rational powers, would generally reach conclusions consistent with, and conducive to, the public welfare.

The contrast between these medieval ideas and the liberal individualist framework is not only striking but instructive. Beyond the inclination of liberalism to doubt the existence of a determinate common good apart from the aggregate goods of individuals, the liberal emphasis upon private rights and rational egoism reverses the medieval concentration upon public duties and moral reason. It might be argued that liberalism is better equipped to protect the individual from domination by the community; but the case for this is weak—empirically, as well as theoretically. On the communal functionalist account, a well-ordered political system must protect and encourage the liberty of its subjects to reason and judge for themselves—and even to judge wrongly, provided that the public good is not harmed thereby. By contrast, for a philosophy that lauds the individual, liberalism demonstrates a remarkable lack of confidence in the ability of each person to employ reason in order to form political judgments. In sum, liberalism's claim to be the unique defender of the individual against the community is difficult to sustain in light of the medieval stress on personal liberty and individual judgment as necessary concomitants of civic life.

It may still be objected that in contrast with liberal individualism, the communal functionalist approach remains far too restrictive in its conception of the diversity of human goods. For example, both John and Marsiglio adopt purely public definitions of function: what counts as a function is whatever contributes to the socioeconomic well-being of the community. This would seem to exclude *private* roles—in particular, activities associated with the family—from the arena of the common good and thus to separate those who perform private functions from a place within the political arena. That the upholding of the public/private distinction and, with it, the maintenance of patriarchal beliefs both about the family and the role of women within it was intended by both John and Marsiglio is clear (Nederman 1990; Nederman and Lawson, 1987). Thus, to make communal functionalism viable in a late-twentieth-century context, it is necessary to rethink the distinction between public and private realms as it relates to the definition of the common good. My own judgment is that one can construct a plausible case that so-called private functions in fact fall within the ambit of the common good and hence that they ought to be included within the range of functions that form the basis for the assignment of civic identity.

More troublesome, perhaps, is the specification of how a function contributes to the common good and whether ostensive absence of function is adequate to remove a person from involvement in communal life. Most obviously, this pertains to the person who is unemployed or homeless or who otherwise does not perform any of the functions necessary for the sustenance and health of the social unit. Is such an individual thereby rendered unfit for the exercise of civic responsibilities? The same sort of question can be posed for severely impaired or disabled persons who are unable to undertake the activities which contribute to the common good. Just as significantly, by what or whose standards are determinations to be made as to whether a person (or the function a person performs or can perform) contributes to the benefit of the community?

No more than a tentative response can be offered at present to this complex set of problems. It seems unreasonable to maintain the view that only when actually engaged in some activity that contributes to communal well-being is a person to be counted as a member of the community. Indeed, this would be rather like saying that one only possesses mathematical knowledge when one is performing some mathematical operation. Knowledge is more profitably regarded as a disposition to know rather than as an act of the knower. In an analogous manner, one's status as a member of a political community may be treated as a disposition to engage in some function that contributes to the common good. Indeed, Marsiglio of Padua expresses the matter in essentially this way when he defines the different parts of the civic body as *habitudinem*, that is, dispositions to perform the various functions necessary for the health of the community (1927, 26–27). Consequently, anyone who has a *capability* or *potential* to add in any way to the good of society cannot be placed outside the processes attached to the "politics of the common good." Moreover, only the community itself (either directly or by means of its authorized government) can decide which functions contribute to its well-being. Generally speaking, those which have not been deemed illegal constitute the functions that a communal body accepts as conducive to its own good. This is not to say that mistakes cannot be made in the enforcement of the values common to the society, but such errors have a way of being rapidly overturned and the functions associated with them reinstated. (One thinks of Prohibition Era America, where the legal abolition of the activity of liquor production did not conform to the public perception of its own good and was ultimately negated—in practice by bootlegging and in law by repeal of the Twentieth Amendment to the Constitution.) Communities and their members are quite able to judge the functions necessary for their own welfare. Ironically, it tends to be liberalism, the self-proclaimed proponent of the individual, that doubts this competence.

CONCLUSION

The Latin Middle Ages was by no means the only period of Western history to make use of a functionalist approach to political community. Functionalism is at the core, for instance, of the venerable "guild" tradition of thought (Black 1984; Nederman 1987b). But concentration on diversity has tended to lead in one of two directions antithetical to community: (1) the conflation of function with narrow self-interest, so that different parts are regarded to be essentially in conflict, or (2) the equation of function with hierarchy, so that distinction of task is employed as a principle of subordination and rule. By contrast, medieval communal functionalism as articulated by John of Salisbury and Marsiglio of Padua looks to the diversity of activities as the logical foundation of a community of free individuals. They view function as neither inherently self-regarding nor hierarchical. Rather, reciprocity and interdependence, the necessary foundations of a self-sustaining community, flow from a diversity of members or "offices" within the polity.

That this position forms a distinctive feature of medieval thought may result from the conjunction of a series of largely incongruent intellectual and historical factors. By the twelfth century, the classical and "Aristotelian" (MacIntyre 1985, 146) notion of the common good as the primary and animating feature of public life was in wide circulation among medieval thinkers, one by-product of a general reawakening of interest in ancient philosophy, history, and law (Benson and Constable 1982). This was only reinforced by the recovery and translation of Aristotle's *Politics* in the midthirteenth century. The learned person of the Middle Ages confronted a range of classical sources that focused upon communal identity and the dependence of virtue upon community. At the same time, Christian teaching about the individual's will and the centrality of one's personal relationship with God pulled in the opposite direction: ultimate virtue and happiness arose out of individual judgment and choice, rather than out of the dictates of any common good. Thus, a command based on the public welfare to violate the tenets of one's religious faith was to be met with refusal and (passive) resistance. The individual's will or discretion was deemed to be sovereign in relation to the external pressures and dictates of the community. As MacIntyre has recognized, medieval authors were confronted with "a culture in which human life was in danger of being torn apart by the conflict of too many ideals, too many ways of life" (1985, 165). This cultural situation was compounded by the diversity of social and political arrangements, of economic activities and legal powers (the particularization, or "parcellization," of authority, as historians often call it), which produced tensions that no thinker of the Latin Middle Ages could ignore (Fasolt 1991, 80–83). It was unthinkable to sweep away the divisions within the medieval world; rather, if a civic identity was to be fashioned, it had to start with the historical reality of disparity.

The attraction of the communal functionalist framework was its capacity to cope coherently with these demands upon political and social thought. It inserted the individual into a communal setting while maintaining a meaningful role for personal volition and achieved this precisely by turning diversity of function (divergence in "ways of life") into a virtue—indeed, a necessity. In a certain fashion, the problems confronted by contemporary political theory are not that distant from those which faced John or Marsiglio. Individualism (albeit of a secular sort) constantly resists efforts to identify a common good capable of stimulating expressions of civic virtue; yet the appeal of individualism is spurred on by the unhappy collectivist consequences of too great a concentration upon communal needs at the expense of personal choice and judgment. This similarity of predicament, as well as the scope and power of the communal functionalist model itself, commends a reading of authors such as John of Salisbury and Marsiglio of Padua to current communitarian theorists. Such a reading may bear surprising fruit.

Notes

Versions of this research have been presented to the 1992 conference of the Western Political Science Association and the 1992 meeting of the Canadian Political Science Association, as well as to a philosophy department staff seminar at Siena College. The comments and suggestions of Kate Forhan, Mary Meany, Paul Santilli, and Sara Monoson have been invaluable in sharpening my claims.

1. The term *communitarian* has been applied to theorists whose ideological and philosophical commitments vary so widely that those included under the label resist treatment as members of a "school of thought" advocating a core set of doctrines. See the overviews by Gutmann (1985) and Lasch (1988). Communitarianism most accurately denotes a theoretical concern for the community as a significant force in the political experience of individuals or as a constitutive feature of human identity. Beyond this and a consequent aversion or opposition to liberal individualism, few generalizations seem possible. Indeed, Charles Taylor suggests that it will probably be necessary to dispense entirely with such labels as *communitarianism* (as well as *liberalism*) before the range of philosophical issues at stake can become fully transparent (1989, 163).

2. Essentially the same doctrine, for instance, has recently been ascribed to William Durandus the Younger, an early fourteenth-century theorist, by Fasolt (1991, 179–84).

3. The first citation is to the Latin text, the second to my translation of it.

4. John, in fact, recognizes that the comparison of the feet to the trades and professions associated with artisans, craftsmen, and workers generally is imperfect if only because of the diversity of these activities: "There are so many of these occupations that the number of feet in the republic surpasses not only the eight-footed crab, but even the centipede; one cannot enumerate them on account of their large quantity . . . because there are so many varied kinds that no compiler of duties ever produced special precepts for each individual type" (1909, vol. 2, 59; idem 1990, 126). He is thus far more appreciative of, and sympathetic to, the actual functions and importance of the lesser members than is sometimes credited.

5. Part of the explanation for this may be Marsiglio's class self-identification. Trained as a physician, he counts his own occupation among the trades associated with the "mechanical" or "artisan" part of the civic body, albeit perhaps the "architectonic" of that part (1927, 17). In turn, mechanics fall within the Aristotelian classification of the *vulgaris*. Since Marsiglio deems himself to be competent to give advice about the good order of the community, it seems unlikely that he would place the *vulgaris* outside of the body of those qualified for citizenship.

6. As Quentin Skinner has argued, some such notion of the connection between liberty and the maintenance of community was at the core of much medieval political discussion (1978, vol. 1, 49–65; idem 1990, 128–34). But Marsiglio perhaps needs to be distinguished here, since he regards liberty in a quite specifically individual sense that is not fully compatible with the more communal, republican-oriented meaning of his contemporaries.

7. Thus, in some ways, communal functionalist theories take over aspects of the pluralist project. The potential for a fruitful relation between pluralist and communitarian thought has been examined by Eisenberg (1991a, 1991b).

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RESEARCH NOTES

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POLITICAL BUSINESS CYCLES IN THE PUBLIC MIND

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Numerous studies have sought to discover political business cycles in macroeconomic variables. Although voters' subjective economic expectations have been shown to influence their electoral decisions, no existing research has attempted to uncover cyclical patterns in citizens' economic expectations. Using survey data, I seek to determine whether expectations shift to benefit the incumbent president's electoral interest. The analyses show that the percentage of the public predicting an economic upturn increases before a presidential election. One explanation for the findings is that voters might extrapolate cyclical expectations from macroeconomic conditions that contain election-driven cycles. Yet the analyses show that expectational cycles still appear when the macroeconomic conditions are held constant. I conclude by drawing an explanation without recourse to macroeconomic cycles.

The topic of the political business cycle (PBC) has attracted continuous attention in the field of political economy. Analysts have examined whether and how the incumbent president manipulates the macroeconomy in order to maximize his (or his party's) probability of reelection.[1] Yet few have analyzed whether there are election-driven cycles in voters' expectations that political scientists think have direct effects on their electoral decisions (Kuklinski and West 1981; Miller and Wattenberg 1985). Many economists dismiss analyses of expectations based on the belief that expectations are not directly observable. Thus, they typically work with *assumptions* about expectations. This tendency has also been apparent in existing PBC studies that have focused primarily on election-driven changes in macroeconomic variables, or macroeconomic PBCs. In contrast, political scientists believe that voters make electoral judgments based on expectations that are subjective and more complex than a single assumption postulates. From this perspective, the PBC literature has offered very few clues to understand the linkage between political manipulation and voters' economic expectations. Thus, by examining expectations directly, we can hope to improve our understanding about the public's expectational behavior pertaining to a presidential election.

My chief objective here is therefore to ask whether there are electoral cycles in citizens' economic expectations, or expectational PBCs. I begin by outlining prominent PBC models and show the dynamics of voters' economic expectations under alternative rationality assumptions. Then, I offer empirical analyses of voters' economic expectations, including unemployment and personal financial expectations and consumer sentiment. The analyses find that expectational PBCs do exist and behave in such a way to benefit the incumbent party.

EXPECTATIONS AND POLITICAL BUSINESS CYCLE MODELS

While the casual idea of political manipulation had been around for some time, Nordhaus (1975) was the first to formalize a nonrational model of the PBC on the bases of traditional Keynesian macroeconomic theory and the median voter theorem. The Nordhaus model assumes that the incumbent administration tries to maximize votes and that voters are not fully rational in evaluating the performance of the incumbent administration. Since voting has little economic value to the individual and the reliable forecasts about the future are costly to acquire, retrospective evaluation based on simple facts (e.g., unemployment, GNP growth, and inflation) may be a reasonable way for many voters to make electoral decisions (Nordhaus 1989, 39). In an election, voters reward the incumbent administration for low unemployment (or high growth) and punish it for high unemployment (or low growth).[2]

Given these assumptions, the model predicts that the incumbent administration engages in antiinflation policy early in the electoral term and stimulates the economy as an election approaches. The results are four-year cyclical patterns for key economic variables: unemployment decreases prior to a presidential election and increases early in the next term, whereas inflation rises only after an election (Thus, such inflation has no adverse electoral consequence for the incumbent.) In the Nordhaus model, voters' economic expectations move in parallel with cycles in macroeconomic variables as voters extrapolate "adaptive" expectations from current and immediate past conditions. Because adaptive expectations adjust slowly to changes in environments, expectations revert gradually to the mean from a cyclical peak and trough.

Notwithstanding the popularity of the Nordhaus model, its nonrationality assumption has been called into question by scholars who believe that voters are essentially prospective and sophisticated and use all available information in evaluating the incumbent and forming expectations. According to Chappell and Keech's (1985) path-breaking study on the subject, voters are able to infer a government's economic policy stance from economic outcomes and judge whether policy is too tight (deflationary) or too loose (inflationary). Such policy inferences can help voters punish the incumbent if preelection policy is too loose. This sensible evaluation of incumbent behavior can effectively eliminate an incentive to manipulate the economy because a cyclical boom manufactured by policy manipulation will be viewed as transitory and will not be rewarded by sophisticated voters.

Yet several theorists have shown that PBCs may not be inconsistent with voter rationality. Although a number of models have been developed in line with rational expectations, the assertion common to these models is that a PBC may occur stochastically if informational deficiency prevents the electorate from correctly forecasting political manipulation. For example, Backus and Driffill (1985) argue that there are two types of administrations defined in terms of the proclivity to stimulate the economy: a "wet" administration that stimulates from time to time and a "hard-nosed" administration that never stimulates. The electorate tries to judge the true preference of the incumbent administration based on its past policy behavior (or reputation). Under these circumstances, the incumbent administration has an incentive to appear to be hard-nosed by not stimulating early in its term and then stimulate in a preelection period. This "unanticipated" policy can generated significant effects on employment and real output prior to an election. Since the electorate learns about the stimulative proclivity of an administration from being tricked once, the administration cannot produce effective political manipulation in two consecutive elections. Hence, a macroeconomic PBC can occur probabilistically, rather than systematically. Another feature of the Backus–Driffill model is that unlike adaptive voters assumed in the Nordhaus model, rational voters can perceive an economic boom as transitory immediately after the actual growth rate accelerates above the natural (or equilibrium) growth rate. Thus, the expectation of a cyclical boom, if it occurs because of stochastic manipulation, will not persist and will quickly dissipate.

Similarly, Rogoff (1990) and Rogoff and Sibert (1988) have developed alternative rational expectations models in which a PBC may occur in government spending and taxes if the incumbent administration has an incentive to manipulate the public's view of the administration's *competency* (defined as the efficiency of providing large amounts of government service at low tax rates). Because such competency can be observed by the public only after a time lag, the incumbent administration has a chance to manipulate fiscal policy before an election in order to appear more competent than it really is. The Rogoff models imply that there may be a real expected (transitory and noninflationary) boom in proximity to an election explainable by a real supply-side effect of a marginal tax cut.

In sum, these PBC models present different scenarios of expectational dynamics under alternative assumptions about voter rationality. Assuming systematically deficient voter information, the Nordhaus model predicts that macroeconomic PBCs may occur frequently in election years. And expectational PBCs may be extrapolated from macroeconomic cycles. In the rational models where voter information is incomplete but only stochastically deficient, macroeconomic PBCs may occur probabilistically, depending on the availability of the information regarding the administration's true objective function. Accordingly, stochastic and transitory cycles may arise in expectations.

EMPIRICAL ANALYSES

Survey data are used to observe citizens' economic expectations directly. The Survey Research Center (SRC) at the University of Michigan regularly asks respondents questions concerning their expectations about future economic conditions.[3] For instance, the SRC has asked the question "Now looking ahead—do you think that a year from now you (and your family living there) will be better off financially, or worse off, or just about the same as now?" and "How about people out of work during the coming 12 months—do you think that there will be more unemployment than now, about the same, or less?" Also, the SRC has produced the Index of Consumer Sentiment, a composite indicator of citizens' general economic outlook, based on multiple questions about their consumption attitudes and various aspects of economic conditions. (The larger the value of the index, the better the economic outlook.) These surveys yield aggregate time series data on citizens' economic expectations, enabling one to observe whether expectational PBCs exist in the recent past.[4]

These expectational series have been chosen because of their electoral significance. Such macroeconomic variables as real personal income and unemployment corresponding to the SRC financial and unemployment expectations series are found to be significantly related to votes in presidential elections or to presidential popularity closely associated with votes (Chappell 1990; Erikson 1989; Fair 1978; Monroe 1984; Hibbs 1987). Also, the consumer sentiment index is shown to be related to presidential popularity (MacKuen, Erikson, and Stimson 1989). Should there be expectational PBCs, such cycles would appear in these three expectational series.

The general equation used to analyze expectations contains predictor variables, including (1) the lagged dependent variable, (2) a partisan variable, (3) economic variables, and (4) an election variable. The lagged dependent variable represents various inertial forces in expectation formation and also justifies serial independence in estimation. The inclusion of a partisan dummy variable is intended to distinguish an election-driven cycle in expectation from a cycle that may be created through partisan alteration in the presidency. Conventional wisdom states that a Democratic administration prefers to reduce unemployment rather than inflation and is willing to accept higher rates of the latter to achieve the former, whereas a Republican administration prefers lower rates of inflation and is willing to pay the price in terms of higher unemployment (Hibbs 1977, 1987). Such a partisan effect is specified as a dummy variable that equals 1 when a Democratic president is in office and 0 otherwise.[5]

Expectations may well depend on current and past economic conditions. The equation includes "information variables" that may be used by the public to form expectations. Unemployment change and inflation are chosen as the information variables because they are frequently reported in media and are matters of considerable public concern.[6] Lags 0–3 of the variables are entered in distributed lag form into the equation without imposing a restriction on the lag structure.[7]

The most important election variable equals 1 for an election quarter and 0 elsewhere. Following Grier (1989), the election-quarter dummy variable takes 15-quarter, second-order Almon polynomial distributed lag (PDL) to estimate a cycle.[8] The PDL technique imposes a smoothing restriction on the shape of a cycle; and the coefficient estimates for the distributed lags represent the values of the mean innovations in a dependent variable for each quarter of a presidential term. The PDL equations for consumer sentiment and personal financial expectations were estimated for the quarterly period between 1961.1 and 1988.4. The equation for unemployment expectations covered a somewhat shorter period between 1965.1 and 1988.4 due to many missing values in the SRC unemployment data in the early 1960s.

Table 1 presents the regression results. In the three equations, the regression fit is reasonable; and, importantly, all individual electoral coefficients are statistically significant by normal standards and appear to constitute a cycle. One relevant issue is whether the seemingly cyclical pattern is significantly different from a single, fixed intercept (or whether the 16 electoral coefficients are equal). To examine this point, the fit of the PDL model was compared with that of an ordinary least squares model that replaced the election-quarter dummy and the distributed lag structure with a single intercept. For the personal finance and the consumer sentiment models, the F-tests could reject the equality restrictions; but, for the unemployment model, the F-test failed to reject the equality restriction.[9] Thus, the interpretation of the unemployment results requires caution.

Figures 1–3 graph the shapes of the electoral dummy variable's lag distributions derived from the three PDL models. In each figure, a presidential term starts at the first quarter, and an election is held at the sixteenth quarter. Each figure is drawn to represent the mean value of a dependent variable for each electoral quarter computed from the estimates of a distributed lag coefficient and the adjustment coefficient for the lagged dependent variable.

The flexible estimates provide comprehensive pictures of shifts in expectations that are consistent with the PBC story. Financial expectations trough at 38.70% in the eighth quarter and peak at 42.81% in the sixteenth, or the election, quarter, whereas unemployment expectations peak at 23.80% in the sixth quarter and trough at 13.83% in the election quarter. Notice that the measurement unit of the two series is the percentages of the public expecting the improvement in personal financial conditions and declines in unemployment, respectively. These movements are electorally significant in creating crucial effects on contemporary presidential elections (e.g., the 1960, 1968, and 1976 elections) where the winners were determined by a few percentage-point differentials in popular votes. Consumer sentiment shows a similar electoral pattern (troughing at 96.27 in the seventh quarter and peaking at 103.89 in the election quarter). Because the consumer sentiment series is a composite index, it is difficult to interpret the precise electoral effect of the change.[10] Equally important, in the three equations, an anticipated economic upturn appears to last for at least six quarters before a presidential election. The persistence of an anticipated boom seems to suggest that citizens' expectations contain substantial inertial forces. This accords with the Nordhaus nonrational PBC model, rather than with the rational models predicting swift mean-reverting expectational behavior.

None of the coefficients for the partisan variable in Table 1 are statistically significant. As far as people's expectations are concerned, an election seems to have a larger effect than partisanship, contrary to the macroeconomy, where partisanship has been shown to exert a strong disturbance (Hibbs 1987). This result should not be too surprising. It seems logical that in an era when partisan loyalty has been on the wane (P. Beck 1984; Clarke and Suzuki 1991), voters might perceive that incumbency-based electoral politics plays a relatively important role in economic affairs. The scope of this argument, however, is limited only to public economic expectations. Suggesting the relative importance of electoral politics in people's subjective expectations does not reject the utility of the partisan theory in explaining political influences on macroeconomic conditions.[11]

As for economic variables, the linear combinations of lagged coefficient estimates are presented because our interest is in the total effects of the variables on expectations, rather than in the precision of each coefficient estimate. The estimates are largely sensible: unemployment change has a negative effect on consumer sentiment, while inflation has negative effects on financial expectations and consumer sentiment. Inflation also has a positive effect on unemployment expectations due to the stagflation of the 1970s, in which inflation and unemployment accelerated together. In the auxiliary analyses, (not shown), where all economic and partisan variables have been dropped from the PDL models in order simply to describe shifts in expectations, the shapes of expectational shifts have remained virtually unchanged. Expectational PBCs still appear, even controlling for economic conditions that may involve electoral cycles. These results imply that expectational PBCs may not necessarily be extrapolated from current economic conditions. Part of the expectational cycles

Table 1

Flexible Estimates of Electoral Effects on Citizens' Economic Expectations

VARIABLE	PERSONAL FINANCIAL EXPECTATIONS 1961.1–1988.4	UNEMPLOYMENT EXPECTATIONS 1965.1–1988.4	CONSUMER SENTIMENT 1961.1–1988.4
Lagged dep. var.	.17	.55**	.44**
	(.10)	(.09)	(.08)
Inflation[a]	−4.29**	4.24**	−6.31**
	(.64)	(1.29)	(1.02)
Unemp. change[a]	.01	.41*	−.39**
	(.08)	(.23)	(.12)
Partisan dummy[b]	−.56	−2.97	.87
	(.64)	(1.68)	(.82)
Lag distr. of elec.-quart. dummy (elect)			
Elect ($t - 15$)	34.54**	10.28**	56.13**
	(4.19)	(3.07)	(8.54)
Elect ($t - 14$)	33.88**	10.54**	55.48**
	(4.09)	(2.89)	(8.39)
Elect ($t - 13$)	33.33**	10.73**	54.94**
	(4.01)	(2.81)	(8.27)
Elect ($t - 12$)	32.88**	10.84**	54.51**
	(3.96)	(2.81)	(8.18)
Elect ($t - 11$)	32.54**	10.88**	54.20**
	(3.92)	(2.85)	(8.10)
Elect ($t - 10$)	32.29**	10.84**	54.00**
	(3.89)	(2.90)	(8.05)
Elect ($t - 9$)	32.16**	10.73**	53.91**
	(3.87)	(2.95)	(8.02)
Elect ($t - 8$)	32.12**	10.55**	53.93**
	(3.87)	(2.98)	(8.00)
Elect ($t - 7$)	32.19**	10.29**	54.07**
	(3.87)	(2.98)	(7.99)
Elect ($t - 6$)	32.35**	9.96**	54.32**
	(3.88)	(2.96)	(8.01)
Elect ($t - 5$)	32.63**	9.55**	54.68**
	(3.90)	(2.92)	(8.04)
Elect ($t - 4$)	33.00**	9.08**	55.16**
	(3.94)	(2.88)	(8.08)
Elect ($t - 3$)	33.48**	8.52**	55.74**
	(3.98)	(2.85)	(8.15)
Elect ($t - 2$)	34.06**	7.90**	56.44**
	(4.05)	(2.75)	(8.24)
Elect ($t - 1$)	34.74**	7.20**	57.25**
	(4.13)	(2.97)	(8.36)
Elect (t)	35.53**	6.42*	58.18**
	(4.24)	(3.17)	(8.50)
R^2	.68	.79	.90
Standard error	3.20	6.66	4.00
Durbin–Watson	2.07	1.97	2.25

Note: Standard errors are in parentheses.

[a] The estimate is the linear combination of the coefficients for lags 0–3.

[b] The dummy variable equals 1 when a Democratic administration is in power and 0 otherwise.

*$P \leq .05$, two-tailed test.

**$P \leq .01$, two-tailed test.

FIGURE 1

Cycle in Personal Financial Expectations

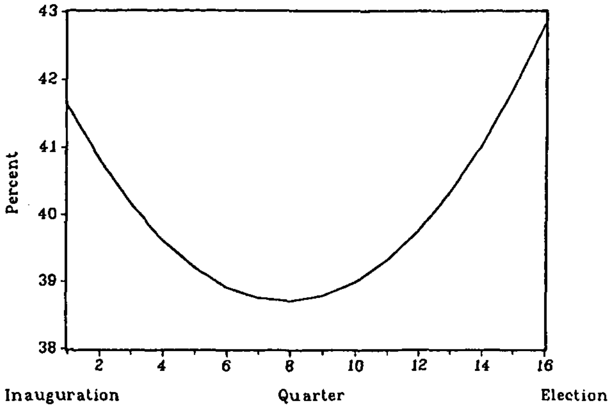
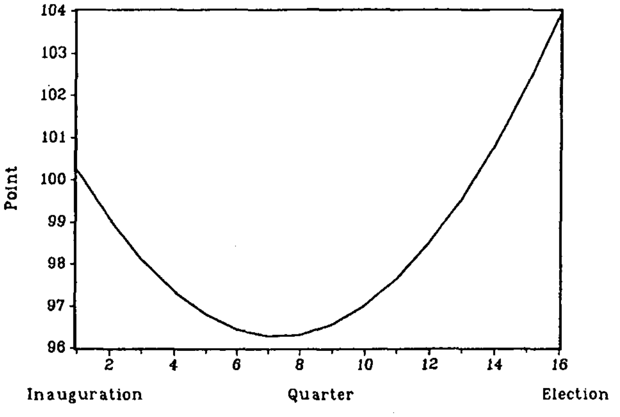


FIGURE 3

Cycle in Consumer Sentiment



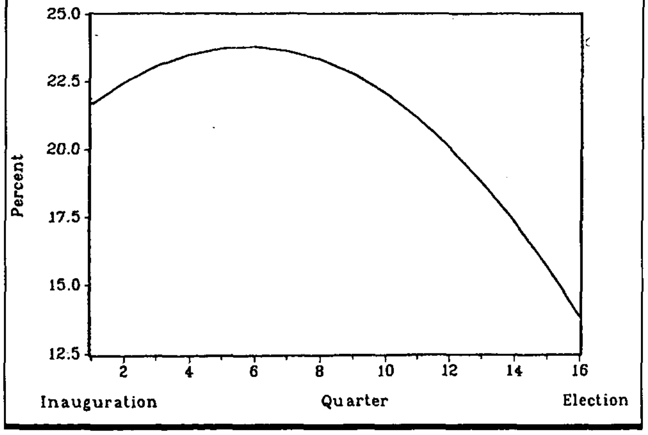
may be attributable to public cognition, independent of outside environments.

To deal with the possibility that these results are an artifact of the statistical techniques employed, I estimated expectational PBCs without distributed lags, using the conventional restrictive specification that models an election impulse, with the *election-year* dummy variable taking the value of 1 for an election year and 0 elsewhere. The ordinary least squares estimates reported in Table 2, columns 1, 3, and 5, support the presence of electoral impulses in the three expectational variables. In all equations, the coefficient for the election-year dummy variable is properly signed and statistically significant, indicating that the electoral patterns in citizens' expectations are insensitive to alternative model specifications.

Both PDL and ordinary least squares analyses involve one potential ambiguity, however. It is not clear whether the expectation of an economic boom is made based on the prospect that the incumbent president (or his party) will stay in power after an election. In contrast, voters may expect such a boom to occur under a new president from the opposition party. To clarify this ambiguity, I have modified the ordinary least squares model by replacing the election-year dummy variable with an interactive term between the election-year dummy and the incumbent party's win probability,[12] predicting a positive sign for this term in the personal financial and consumer sentiment equations and a negative sign in the unemployment equation. The ordinary least squares estimates reported in Table 2, columns 2, 4, and 6, indicate that the coefficient for the interactive term is properly signed as predicted and is statistically significant. The result upholds the hypothesis that the anticipation of an economic upturn can be linked to the incumbent party's win probability.

FIGURE 2

Cycle in Unemployment Expectations



CONCLUSIONS

Unlike previous political business cycle studies examining aggregate macroeconomic variables, my analysis has focused on people's subjective economic expectations that have been shown to influence voting behavior. The search for cycles has been made with SRC survey data using both restrictive and flexible specifications. The results show that financial and unemployment expectations, as well as consumer sentiment, contain four-year cycles adequately timed with a presidential election calendar. And the incumbent party is likely to be an electoral beneficiary of such cycles.

The question is thus, Which political business cycle model can explain these findings best? Because of the limited number of elections examined here, the analyses do not allow us to conclude confidently that the electoral patterns in citizens' economic expectations are either systematic or stochastic or accord either with the Nordhaus nonrational model or the rational models. Nonetheless, slow mean-reverting tendencies found in expectations seems to favor the Nordhaus model. If voters rely on retrospective economic judgments, cycles in their economic expectations may be extrapolated from observations of macroeconomic

Table 2

Restrictive Estimates of Electoral Effects on Citizens' Economic Expectations

	PERSONAL FINANCIAL EXPECTATIONS, 1961.1–1988.4		UNEMPLOYMENT EXPECTATIONS, 1965.1–1988.4		CONSUMER SENTIMENT, 1961.1–1988.4	
VARIABLE	(1)	(2)	(3)	(4)	(5)	(6)
Constant	31.51**	31.16**	10.37**	10.63**	49.89**	49.73**
	(3.83)	(3.82)	(2.72)	(2.76)	(7.49)	(7.56)
Lagged dep. var.	.20*	.21*	.55**	.55**	.49**	.49**
	(.09)	(.09)	(.09)	(.09)	(.08)	(.08)
Inflation[a]	−4.15**	−4.02**	4.23**	4.15**	−5.84**	−5.69**
	(.63)	(.62)	(1.25)	(1.24)	(.95)	(.95)
Unemp. change[a]	.01	.01	.39	.40	−.35**	−.36**
	(.08)	(.08)	(.23)	(.23)	(.12)	(.12)
Election-year dummy[b]	2.20**	—	−3.43*	—	2.88**	—
	(.75)		(1.67)		(.92)	
Partisan dummy[c]	−.58	−.51	−2.97	−3.17	.72	.78
	(.64)	(.64)	(1.64)	(−1.91)	(.81)	(.82)
Interactive term[d]	—	3.78**	—	−5.91	—	4.51**
		(1.30)		(3.06)		(1.61)
R^2	.68	.68	.79	.79	.90	.90
Standard error	3.20	3.20	6.63	6.64	4.00	4.00
Durbin–Watson	2.07	2.07	1.95	1.94	2.27	2.26

Note: Standard errors are in parentheses.

[a] The estimate is the linear combination of the coefficients for lags 0–3.

[b] The dummy variable equals 1 for the four quarters of the election year and 0 otherwise.

[c] The dummy variable equals 1 when a Democratic administration is in power and 0 otherwise.

[d] Election-year dummy times incumbent win probability.

**P* ≤ .05, two-tailed test.

***P* ≤ .01, two-tailed test.

PBCs that are shown to exist by Haynes and Stone (1989) and Grier (1989). The troubling point, however, is that expectational cycles exist independent of any cycle in economic outcomes.

This leads us to believe that expectational PBCs may be driven not entirely by macroeconomic PBCs but, in part, by sources other than policy manipulation. It can be suggested that part of the expectational cycles may be attributed to voters' preelection psychologies. Because of widespread preconceptions about reelection politics and the related media barrage, some voters may anticipate that the incumbent party will try to stimulate the economy before an election, even though they have not yet actually observed an economic upturn. Then, the incumbent party may, in turn, be forced to stimulate, since the failure to do so might result in a recession that is electorally detrimental.[13] Accordingly, such preelection anticipation may drive the electoral manipulation of macroeconomic policy, which, if there is sluggishness (e.g., price stickiness) in the economy, can create cycles in macroeconomic conditions.

I have suggested a new way of thinking about political economy. Political business cycles can, and should be analyzed in conjunction with voters' subjective perceptions that count in elections. I have shown that the politicoeconomic forces that influence voters' political decisions are vigorous and are here to stay.

Notes

An earlier version of this paper was presented at the annual meeting of the Midwest Political Science Association, Chicago, 1991. I thank Henry Chappell, Harold Clarke, John Londregan, and William Mishler for constructive comments.

1. Researchers made numerous efforts to search for PBCs in a variety of economic variables, including inflation, unemployment, real gross national product, and monetary and fiscal aggregates. Their analyses have produced mixed results regarding the presence of macroeconomic political business cycles in contemporary U.S. history. (Negative results are found in N. Beck 1982; Golden and Poterba 1980; Hibbs 1987, chap. 8; and McCallum 1978. Positive results are found in Grier 1987, 1989; Haynes and Stone 1988, 1989; Laney and Willett 1983; Maloney and Smirlock 1981; and Tufte 1978. See Alt and Chrystal 1983, chap. 5 and Nordhaus 1989 for reviews of empirical political business cycle studies.)

2. Econometric studies of votes in presidential elections initiated by Fair 1978 and subsequently replicated (Brody and Sigelman 1983; Hibbs 1987, chap. 6; Lewis-Beck and Rice 1982; Tufte 1978) found that voters reward the incumbent party for high economic growth and punish it for low growth. More recently, using improved model specifications Chappell (1990) and Erikson (1989) show that economic effects on votes are even stronger than Fair originally reported. These results

provide a rationale for the party in power to manipulate the business cycle for electoral advantage.

3. Using an equal probability sample design and telephone interviews, the SRC surveys over one thousand adults every month. Based on these data, the SRC produces the quarterly series employed in this study. The SRC notes that its survey involve sample errors of ±5%. Sample errors do not confound the time-series analyses presented in the text because errors remain relatively constant across samples.

4. The literature on voting behavior is not clear about how far ahead voters look in making electoral decisions. The one-year time horizon used in the SRC data seems reasonable because it is very difficult to predict future economic performance beyond this time horizon.

5. Alesina (1987); Alesina, Londregan, and Rosenthal (1990); Alesina and Sachs (1988); and Chappell and Keech (1988) claim that partisan effects may be transitory because markets are presumably able to return quickly to equilibria in a rational expectation environment. Rational voters may predict a partisan-driven economic boom based on the likelihood of the Democratic party's winning a presidential election. Thus, the rational partisan variable equals Democratic win probabilities that were computed from presidential approval. This computing procedure is similar to the one used to calculate incumbent win probabilities (see n. 12). The equation replacing the Hibbs partisan dummy variable with the rational variable also was estimated. The results showed that the rational partisan variable is not statistically significant.

6. Unemployment change is measured as percentage change in the seasonally adjusted quarterly unemployment rate (HURR). Inflation is measured as percentage change in the seasonally adjusted quarterly consumer price index. These economic data were obtained from the CITIBASE data tape. Other than the economic variables included in the model, I also tested macroeconomic policy measures such as M1 money supply growth and change in cyclically adjusted fiscal surplus/deficit. Neither policy measure was significantly related to the dependent variables. Thus, these variables were not included in the equations estimated in the text.

7. This particular lag length was chosen because people are shown to use approximately a one-year backward time horizon in forming their expectations (Feige and Pearce 1976; Figlewski and Wachtel 1981; Gramlich 1983).

8. Using a grid search over 1–4, the second-order polynomial distributed lag was chosen as the value that could maximize the adjusted R^2 of the regressions. The use of the second-order PDL model turned out to be consistent with Grier's (1989) estimated macroeconomic PBCs.

9. For the personal finance and the consumer sentiment equations, the test statistics were computed as $F_{2,99} = 4.70$ and $F_{2,99} = 4.34$, respectively (both are significant at the .05 level), rejecting the null hypothesis that the 16 electoral coefficients are equal. For the unemployment model, the test statistic was calculated as $F_{2,83} = 1.80$ and was not significant, failing to reject the null hypothesis at the .05 level.

10. In the working paper version of this article, I examined whether, on average, the public underestimates inflation that might rise after an election due to preelection policy stimulation. Using the SRC question about the 12-month-ahead expected inflation rate, inflation expectation errors for the average public were calculated as the deviations of the mean expected rates from the actual inflation rates. Then, a cycle in the errors was estimated via the PDL technique. The results indicated that the distributed lag coefficients are consistently positive and are significant during the first eight quarters of a presidential term. This implies that in a postelection period, actual inflation rises faster than the electorate expects but subsides and converges gradually to public expectations as an election approaches.

11. Nordhaus (1989) argues that electoral and partisan manipulations may well coexist in macroeconomic conditions.

12. The incumbent win probabilities were calculated using the procedure developed by Chappell and Keech (1988). I first regressed the actual shares of votes gained by the incumbent party on presidential approval shares in election quarters. From this regression I constructed a predicted vote share. I next calculated the forecast error for the prediction. Given the prediction and its forecast error, I calculated the incumbent's win probability, which was assumed to equal the probability that the incumbent vote share would exceed one-half. I proceeded in similar fashion to compute one-quarter-ahead win probabilities, using approval shares in various quarters before election quarters.

13. See Barro and Gordon (1983) for a theoretical treatment of the reputational game between the government and the public.

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COURTING CONSTITUENTS? AN ANALYSIS OF THE SENATE CONFIRMATION VOTE ON JUSTICE CLARENCE THOMAS

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The increasing public attention paid to Supreme Court nominations has elevated the salience of Senate confirmation battles, raising interesting questions about the impact of constituency preferences on senators' voting behavior. In this article, we explore this relationship using a logistical regression model to examine the impacts of African-American constituency size and the proximity of reelection on the roll call behavior of senators on the Clarence Thomas confirmation vote. Our analyses indicate that these factors were both statistically and substantively significant in the Thomas case. We conclude by discussing the theoretical and practical implications of such findings.

The factors that contribute to Senate confirmation votes on Supreme Court nominees have long fascinated scholars of both institutions. While partisan, ideological, and institutional variables are those most often cited, recent events suggest that some senators under certain circumstances are influenced by the preferences of their constituents. In this article, we take advantage of events surrounding the 1991 nomination of Judge Clarence Thomas to test this hypothesis. Our findings support the conclusion that constituency concerns played a significant part in Thomas's eventual confirmation and imply that further scholarly investigation of this constituency connection is warranted.

PREVIOUS RESEARCH

As a major player in the political process, the U.S. Supreme Court renders decisions that often have considerable impact on major areas of American life. While its decisions are periodically reversed, sometimes limited, and occasionally ignored by the other branches, the court wields significant power, particularly for an institution comprised of so few individuals.

Because the court's composition is a critical determinant of the policies it produces and because justices enjoy a life tenure on the bench, presidential nominations and Senate confirmations are important decisions whose repercussions echo for decades. Moreover, the appointment of Supreme Court justices takes on added importance in an era of divided party government, when representatives of the two parties struggle for control of the judicial branch. In such circumstances, the composition of the court is not only substantively important in terms of policy outputs, it is also a matter of considerable political interest.

Over the years, scholars have paid considerable attention to both the criteria upon which presidents base their nominations (e.g., Abraham 1985; Ackerman 1988; Daniels 1978; Fish 1988–89; Hulbary and Walker 1980; Perry 1991; Schmidhauser 1979; Scigliano 1971) and the "advise and consent" powers of the Senate (Grossman and Wasby 1972; Lively 1986; Nagel 1990; Rader 1988–89). Much related literature has addressed this branch interaction, seeking to illuminate the conditions under which presidents are likely to win Senate confirmation of their judicial nominees. Some examinations have focused on the overall political environment, especially factors related to the partisan balance of power. Halper (1972), for example, concluded that the Senate's opposition to a nominee is usually part of a larger pattern of opposition to the president, making the Senate more likely to reject a nomination put forth by an electorally weak or unpopular president. Others have expanded on this point, arguing that presidents who are late in their terms of office or who lack strong party backing in the Senate have more difficulty winning confirmation of their nominees (Palmer 1983; Scigliano 1971, 97–99; Segal 1987; Segal and Spaeth 1986). It has even been suggested that during periods when the court itself is unpopular, the Senate is more likely to reject nominees, particularly if such unpopularity coincides with presidential weakness (Halper 1972, 111–12; Schmidhauser and Berg 1972, 128).

In addition to examining the Senate's decisions on judicial confirmations, scholars have also scrutinized the votes of individual senators. Much of this research has substantiated the link between senators' ideologies and their voting behavior, especially on controversial nominations (Rohde and Spaeth 1976, 105–6; Songer 1979; and Sulfridge 1980). While partisanship and region have also been shown to have an impact on some confirmation voting, most recent work has concluded that ideology consistently has a larger effect on senators' voting behavior on controversial nominations (see Felice and Weisberg 1988–89).

Building on such work, Segal, Cover, and Cameron (1988–89) have suggested that senators' ideologies interact with environmental factors to affect nomination outcomes. Specifically, they concluded that ideological distance between a senator and a nominee plays an important role in the senator's voting decision only when the nominee's qualifications have been questioned or when the political environment works against the president (i.e., when the opposition party controls the Senate or when he is in the fourth year of a term). They have also suggested that the behavior of electorally-minded senators is sensible and predictable: "When a strong president nominates a highly qualified, ideologically moderate candidate, the nominee passes the Senate in a lopsided, consensual vote"; on the other hand, when a weak president nominates a "less well qualified, ideologically extreme candidate," a closer, more conflictual Senate vote is likely (Cameron, Cover, and Segal 1990, 532; see also Massaro 1990).

Recently, some scholars have begun to suggest that there is also a "constituency perspective" to Senate voting on Court nominees (Felice and Weisberg 1988–89). This point has been articulated most forcefully by Caldeira, whose review of the literature in this area concluded that "more attention ought to be given to the influence of constituencies and mobilization by organized interests" (1988–89, 532).[1] Segal, Cameron, and Cover have recently addressed one aspect of the constituency connection; examining the relationship between constituents' ideological characteristics and the ideologies of Supreme Court nominees, they conclude that "confirmation voting is decisively affected by the ideological distance between senators' constituents and [Supreme Court] nominees" (1992, 109).

Such findings, of course, comport well with much general research on the Congress, where the received wisdom has long held that constituency preferences influence many aspects of congressional behavior (for authoritative statements see Fenno 1978; Fiorina 1977; and Mayhew 1974). Although some have questioned the impact of constituency on issues where members have strong ideological predispositions (Bernstein and Anthony 1974; McCormick and Black 1983; Moyer 1973), the weight of legislative research seems to agree that strong constituency preferences on salient issues will significantly affect congressional action either at the roll call stage or at some earlier, agendasetting stage (Arnold 1990; Overby 1991). In the area of judicial confirmations, attention to such a constituency dimension has been limited, Caldeira argues, due to difficulties in operationalizing measures of constituency effect and to the fact that court nominations have only recently been the subject of widespread public interest (1988–89, 537).

For several reasons, the 1991 confirmation battle over Clarence Thomas provides an excellent occasion to extend the insights of these researchers and explore in some depth the effects of constituency preferences on Senate confirmation votes. In addition to being a contested, nonconsensual nomination, the struggle over confirmation was of particular interest to an easily identifiable constituency, namely, African Americans. The fact that the size of this constituency varies considerably among the states (from .22% of the population in Vermont to 35.2% in Mississippi) permits testing its effect in a multivariate regression model. What is more, the fact that most organized African–American interest groups opposed the nomination (Biskupic 1991, 2692) while polls showed clear majorities of African–American respondents supportive of Thomas (Moore 1991) permits us to differentiate between the effects of organized interests and unorganized interests (constituencies).

Before turning our attention to the test of the constituency hypothesis, however, it is worth briefly reviewing the details of the Thomas nomination and confirmation.

THE THOMAS CONFIRMATION

On 1 July 1991, President George Bush nominated Clarence Thomas to fill the Supreme Court seat left vacant by the retirement of Associate Justice Thurgood Marshall. At the press conference announcing the nomination, Bush characterized Thomas as the "best person for this position" and denied that the nominee's race or his desire to fill a racial quota on the court had played any part in his choice ("Bush Announces" 1991).

A graduate of Yale Law School and a former chair of the Equal Employment Opportunity Commission (EEOC), Thomas was a judge on the U.S. Court of Appeals for the District of Columbia Circuit when Bush nominated him for the high court. Given his conservative political ideology and his rather turbulent tenure at the EEOC (where he had been criticized for allowing hundreds of age discrimination complaints to lapse without action), it was not surprising that there was significant opposition to his nomination from the beginning. In the Senate, some Republicans and many Democrats indicated they were unlikely to support a nominee who evaded questions or remained silent on the abortion issue (Biskupic 1991b, 1826). Outside the Senate, groups advocating abortion rights, women's rights, civil rights, and senior citizens rights urged the rejection of the Thomas nomination.

Thomas, however, was not without considerable advantages and support. His background, frequently characterized as a self-motivated ascent from childhood poverty in rural Georgia to the corridors of political power, evoked considerable national admiration. Likewise, his genial and composed personality mitigated the fears of some that he was ideological, extreme, and insensitive—another Robert Bork. Moreover, he enjoyed the strong support of Senator John Danforth (R-MO), a powerful figure in the chamber, for whom Thomas had once worked. Heading into the Judiciary Committee's hearings, Senator Dale Bumpers (D-AR) seemed to capture the Senate's emerging consensus when he noted, "Regardless of how I'm going to vote on [Thomas], I feel comfortable he's going to be confirmed" (quoted in Biskupic 1991c, 2169).

Although he clearly did not please many liberal members of the Judiciary Committee, five days of hearings provided little reason to question the wisdom of Bumpers' assessment. The majority of questions concerned Thomas's views on abortion (he acknowledged a right to privacy but refused to commit himself on whether or not it extended to abortion), natural law (he indicated that his interest was political and did not affect his judicial philosophy), and civil rights (he testified that he preferred individual incentives to group remedies) (Biskupic 1991a, 2621–23).

On 27 September the committee split evenly (seven to seven) on its vote regarding the Thomas nomination. With one exception (Democrat Dennis DeConcini of Arizona), the vote cut along party lines. The Democrats who voted against him cited Thomas's reluctance to discuss *Roe* v. *Wade* and his equivocations on natural law as critical to their decisions. Although the split vote meant that Thomas's name would be forwarded to the full Senate without a recommendation of confirmation, most observers concluded that Thomas had gained momentum during the hearings and that the full Senate was likely to vote in favor of his appointment in early October.

Before the full Senate could act, however, the confirmation process took a very unusual turn. Reporters learned the identity of a woman who had accused Thomas of sexual misconduct during his service at the EEOC; the leaking of the story persuaded her to come forward and convinced the Senate to delay its vote. On 11 October, in front of the Judiciary Committee and before a nationwide television audience, Anita Hill, a professor of law at the University of Oklahoma and a former aide to Thomas at both the EEOC and the Department of Education, accused the nominee of sexual harassment. She alleged that Thomas had repeatedly made unwanted sexual advances to her and regularly included graphic sexual references in their workplace conversations. In his rebuttal, Thomas angrily denied these allegations and accused the committee of engaging in "a high-tech lynching for an uppity black who in any way deigns to think for himself" (quoted in Biskupic 1991d, 2948).

In the end, Hill's testimony failed to derail the Thomas nomination. After several days of extra hearings notable for their tension and a day full of angry floor speeches, on 15 October, by a 52 to 48 vote, the Senate confirmed Thomas to the Supreme Court. In the closest confirmation vote in over a hundred years, only 2 Republicans voted against Thomas, while 11 Democrats, mostly Southerners, voted in favor of his appointment.

MODELS AND ANALYSIS

With that background, we are now ready to examine the struggle over the Thomas nomination from the "constituency perspective," testing to determine whether the preferences of African-American constituencies in the states affected the confirmation voting behavior of senators.

Since the dependent variable we are interested in—the confirmation vote on Clarence Thomas—is dichotomous, we employ a logistical regression model to test the influence of constituency pressure. To control for the effects of senators' partisan and ideological predispositions, our independent variables include a dichotomous variable for PARTY (coded 1 for Democrats, 0 for Republicans) and each senator's Americans for Democratic Action (ADA) score from 1990.[2]

To test for constituency effects, we consider three variables. The first (RACE) is demographic—percentage of the state population that is African-American based on 1980 census data. Second, since previous research has shown that senators tend to pay greater attention to constituency matters as their reelection dates approach (Fenno 1982; Thomas 1985) and it is reasonable to assume that senators from states with large African-American populations might be especially sensitive to constituency opinion on the Thomas nomination, we include both an electoral proximity variable (REELECT, coded 1 for senators facing reelection in 1992, 0 otherwise) and an interactive variable (RACE AND REELECT) calculated by multiplying the state racial percentage variable by the REELECT variable. Together, these variables will allow us to test several components of possible constituency effects.[3]

Since we know from the literature that party affiliation and ideology should both have a large impact upon senators' voting decisions, we first model those two independent variables alone:

$$\text{Log}\ (\text{VOTE}/1 - \text{VOTE}) = a + b_1(\text{ADA}) + b_2\ (\text{PARTY}). \quad \textbf{(1)}$$

We regress the log odds of each senator's vote on ADA score and party affiliation, allowing for an intercept (a). The results of this test (reported in Table 1, col. 1) are predictable. The coefficients for ideology and partisanship are in the predicted direction (negative, indicating that Democrats and those with liberal ideologies tended to vote against confirmation); and when compared to their standard errors both are statistically significant at the .01 level.[4] In terms of model fit, these two variables permit the correct prediction of 88% (85/97) of the votes and reduce the error in predictions by 74.5% over a "naive" model that uses the modal vote as its prediction for all votes.[5]

To test for the direct effect of state racial composition on the confirmation vote, we add the racial demographic variable:

$$\text{Log}(\text{VOTE}/1 - \text{VOTE}) = a + b_1(\text{ADA}) + b_2(\text{PARTY}) + b_3(\text{RACE}). \quad \textbf{(2)}$$

While the results (Table 1, col. 2) are not dramatic, they are significant. The coefficient for race is in the

Table 1.

Logit Equation Predicting Senate Confirmation of Clarence Thomas

VARIABLE	EQUATION 1	EQUATION 2	EQUATION 3
ADA[a]	−.101** (.026)	−.091** (.027)	−.111** (.034)
PARTY[b]	−1.269** (.516)	−1.841** (.621)	−1.930** (.707)
RACE[c]	—	.103* (.057)	.022 (.067)
REELECT[d]	—	—	−2.195* (1.160)
RACE AND REELECT[e]	—	—	.277* (.139)
CONSTANT	6.233** (1.637)	4.861** (1.748)	4.872** (1.843)
% predicted correctly	87.6	89.6	93.8
Reduction of Error (%)	74.5	78.7	87.2

Notes: Figures in parentheses are standard errors; N = 97.
[a] Senator's Americans for Democratic Action voting score for 1990.
[b] Senator's party (1 = Democrat, 0 = Republican).
[c] Percentage of African Americans in state's total population (based on 1980 census figures).
[d] Senator's reelection status (1 = up for reelection in 1992, 0 = otherwise).
[e] Interactive variable calculated by multiplying RACE times REELECT.
*$p < .05$, one-tailed test.
**$p < .01$, one-tailed test.

Table 2.

Ordinary Least Squares Equations Predicting Senate Confirmation of Clarence Thomas

VARIABLE	EQUATION 1	EQUATION 2	EQUATION 3
ADA[a]	−.009** (.001)	−.008** (.001)	−.008** (.001)
PARTY[b]	−.288** (.093)	−.374** (.099)	−.399** (.095)
RACE[c]	—	.007* (.003)	.000 (.003)
REELECT[d]	—	—	−.222** (.082)
RACE AND REELECT[e]	—	—	.022** (.006)
CONSTANT	1.128** (.051)	1.053** (.061)	1.138** (.066)
Adj. R^2	.68	.70	.73

Notes: The first figures reported are unstandardized regression coefficients (*B*s); the figures in parentheses are their standard errors. N = 97.
[a] Senator's Americans for Democratic Action voting score for 1990.
[b] Senator's party (1 = Democrat, 0 = Republican)
[c] Percentage of African Americans in state's total population (based on 1980 census figures)
[d] Senator's reelection status (1 = up for reelection in 1992, 0 = otherwise)
[e] Interactive variable calculated by multiplying RACE times REELECT
*$p < .05$, one-tailed test.
**$p < .01$, one-tailed test.

predicted direction (positive, indicating that senators from states with large African-American populations tended to vote in favor of confirmation); and when compared to its standard error, it is statistically significant at the .05 level. More importantly, there is improvement in model fit, as the percentage of votes predicted correctly rises to 90% (87/97) and the error declines.

Finally, we construct our most sophisticated model of constituency influence in equation three by adding the reelection term and the interaction variable between race and electoral proximity, in effect testing whether or not state racial composition is most important to senators facing an impending reelection campaign:

$$\text{Log}\ (\text{VOTE}/1 - \text{VOTE}) = a + b_1(\text{ADA}) + b_2(\text{PARTY}) + b_3(\text{RACE}) + b_4(\text{REELECT}) + b_5(\text{RACE AND REELECT}). \quad (3)$$

As before, this more sophisticated model of constituency effect enhances the fit. Not only is the coefficient for the interactive term in the predicted direction (positive, indicating that senators facing a large African-American constituency in an impending campaign were more likely to vote in favor of confirmation), it is also significant at the .05 level. Interestingly, while the interactive coefficient is positive and significant, the coefficient for REELECT alone is negative and significant. This is perhaps an indication of the cross pressure many senators—especially those up for reelection—must have felt, caught uncomfortably between African-Americans, many of whom saw in Thomas a role model facing a "high-tech lynching", and others who were concerned about his suitability for the Court because of both his policy stances and Anita Hill's allegations of sexual harassment. Taken together, these additional variables improve the fit in comparison to the naive model, contributing to a better predictive capability of 94% (91/97) and a reduction of error value of over 87%.

Viewed together, the three equations summarized in Table 1 show that racial constituency characteristics consistently exercised a predictable, independent, and significant effect on senators' voting behavior and that there is a steady improvement in model fit as we treat the constituency effect in an increasingly sophisticated fashion. In comparison to equation 1, equation 3 cuts the number of incorrectly predicted votes in half (from twelve down to six) and reduces the error over the naive modal model an additional 12.7%.

Since the interpretation of logistic regression output can be difficult, we also modelled the equations using ordinary least squares. Although we technically violate this method's assumptions by using a constrained dependent variable, the closeness of the vote (52 to 48) mitigates the seriousness of this problem. The recalculations of each equation are presented in Table 2.

While we can have less confidence in the actual significance of the coefficients reported here, these findings generally reinforce those reported in Table 1. Race and the interactive race–reelection variables add to the explanatory power of the model in equations 2

Table 3.

Mean Probability of Vote to Confirm for Moderate Democrats* Under Certain Constituency Conditions

%AFRICAN-AMERICAN IN POPULATION	EQUATION 2 (RACE Alone)	EQUATION 3 (RACE AND REELECT) FACING REELECTION	EQUATION 3 (RACE AND REELECT) NOT FACING REELECTION
0	.19	.01	.07
10	.39	.15	.08
20	.64	.78	.11
30	.83	.99	.12

*For illustrative purposes, a "moderate Democrat" is defined here as one with an ADA score of 50.

and 3, respectively. Compared to equation 1, equation 3 increases the proportion of variance explained (adjusted R-squared) from .68 to .73.

The substantive impact of constituency concerns can better be understood by examining the behavior of specific senators. Compared to equation 1, equation 2 (which adds race) correctly predicts the *yes* votes of senators Ernest Hollings (D-SC) and Sam Nunn (D-GA), both moderately conservative Southern Democrats representing states with sizable African-American constituencies (30% and 29%, respectively). Likewise, compared to equation 2, equation 3 (which adds the reelection and the interactive race-reelection variables) correctly predicts the *no* votes of senators Wendell Ford (D-KY) and Robert Packwood (R-OR). Both of these senators are up for reelection in 1992, yet represent states with relatively small African-American constituencies (7% and 1.5%, respectively), suggesting that they responded to constituents mobilized against Thomas's confirmation. In addition, equation 3 correctly predicts the *yes* votes of senators Wyche Fowler (D-GA) and Mark Hatfield (R-OR). While Hatfield's case is difficult to understand, Fowler's is not. He is predicted correctly by this equation because, although he is a moderately liberal Southern Democrat (ADA = 72), he stands for reelection in 1992 *and* represents a constituency that is 29% African-American.[6] In short, adding constituency variables to the model not only aids in the prediction of votes, but does so in ways that are generally substantively interesting.

The substantive impact of constituency preferences is also highlighted by Table 3, which recalculates the logit regression coefficients generated in Table 1 for a hypothetical moderate Democrat under a variety of constituency circumstances and generates the probabilities of a *yes* vote under such conditions.[7] In the second column, the effect of race is considered alone; and the results are marked. A moderate Democrat from a state with an African-American population approaching zero had only a one-in-five probability of voting for confirmation. As the percentage of African Americans in the population rises to 10%, 20%, and 30%, the senator's likelihood of voting for Thomas increases to 39%, 64%, and 83%, respectively.

The third and fourth columns of Table 3 compare the hypothetical moderate Democrat's probability of voting to confirm, given the significant interactive effect between race and reelectoral proximity. For the senator facing reelection, the effect of race is striking. Such a senator's likelihood of voting for Thomas increases from 1%, to 15%, to 78%, as the percentage of African-Americans in the state constituency rises from 0%, to 10%, to 20%. At 30% African-American population, a moderate Democrat facing reelection is 99% certain to vote for Thomas.

CONCLUSIONS

While the centrality of constituency concerns has long been acknowledged in much literature on the Congress, only recently have scholars suggested that such concerns might affect one of the most important arenas of congressional decision making—Supreme Court confirmations. Moreover, until recently, the historical coincidence of reasonably noncontroversial votes and constituency connections that were difficult to operationalize conspired to make the testing of such a hypothesis nearly impossible. The nomination of Clarence Thomas, however, changed that. With the closely contested, highly controversial confirmation vote and the easily identifiable constituency groups interested, this nomination struggle provides an excellent opportunity to investigate the impact of constituencies on senators' voting behavior.

Our findings in this study extend previous research along an important new dimension that has both theoretical and practical consequences. By demonstrating how, and under what circumstances, constituency characteristics influenced the voting behavior of senators, we have substantiated Caldeira's claim and established that constituency considerations can significantly affect confirmation outcomes, at least in highly salient cases. This suggests that our general models of the process need to be amended to account for this factor. In the absence of such emendations, current models limited to institutional, ideological, and partisan variables will systematically fail to predict the behavior of certain senators—those who for electoral and demographic reasons have cause to be most sensitive to constituency opinion. Furthermore, we extend the analysis of Segal, Cameron, and Cover (1992) by demonstrating that constituency preferences can influence confirmation voting behavior without working through the intervening factor of ideological distance from the nominee.

These findings carry at least two practical political implications. The first concerns the "representative" nature of the court. In an era of divided party government, Republican presidents and their judicial nominees are likely to face greater hostility and closer scrutiny from Democratic Senate majorities than ever before. Facing such a situation, strategic considerations counsel in favor of recommending nominees who have the support of a political constituency important to key, "swing" senators. Such was clearly the case with Thomas, a conservative African–American who had considerable appeal to several constituencies critical to the electoral fortunes of moderate, Southern Democrats. Whether other demographic groups could exercise the same pressure on the Senate is unclear, but certain groups (especially Hispanic Americans) at least have that potential. More generally, our findings raise the interesting possibility that in an age of televised confirmation hearings and glaring public scrutiny of nominees, "going public" to raise widespread popular interest and bring constituent pressure to bear on wavering senators might be one of the more productive strategies available to the White House in confirmation struggles with the Senate.

Second, in terms of the Thomas confirmation itself, knowledge of the constituency dynamic would have been—or, indeed, probably was—of considerable help to the White House in terms of predicting the outcome of the Senate vote. Operating with just the knowledge of individual senators' ADA scores and party, the White House could have predicted that Thomas would have been confirmed but only by the slimmest of margins—49 to 48, perhaps a close enough call to pull the matter from consideration, at least cause enough for extraordinary White House lobbying pressure or Republican parliamentary delays. With knowledge of the constituency effect, however, the Bush administration could rest easier, more confident of victory. Our full model of the confirmation vote, including ideology, party, race, electoral proximity, and the race–proximity interaction, predicts a 51-to-46 final vote, which is very close to the actual vote of 52 to 48.

In the final analysis, it is unlikely that constituency concerns will ever replace ideological predispositions and partisan loyalties as the principal determinants of Senate confirmation votes. In an age of divided party government, however, when such votes promise to be less consensual, constituency concerns may be of considerable significance in terms of the results of close contests. Given the outcome of the Thomas nomination, it seems likely that both strategic presidents and rational senators will pay close attention to constituency angles in future nomination battles.

Notes

We would like to thank Mark Chaves and John Frendreis for their insightful suggestions on earlier drafts of this article.

1. Caldeira differentiates between constituencies and "organized interests" by noting that the former are "unorganized interests, such as blacks, farmers, women, etc." (1988–89, 532n.).

2. In the case of Senators Brown (R–CO), Craig (R–ID), and Smith (R–NH) we employ 1990 ADA scores from the House of Representatives. Senators Seymour (R–CA) and Wofford (D–PA), both recently appointed, and Wellstone (D–MN), recently elected, without prior congressional service, have no ADA scores, and hence are generally excluded from the analysis, rendering the total number of cases 97 votes. We do not include a measure of presidential support in the model because it correlates highly with ADA scores (at approximately .8) and therefore produces multicollinearity problems in the model. In addition, the most readily available measures of presidential support are widely criticized in the literature (see Edwards 1989, chap. 2, for a summary).

3. We do not include a regional variable in our model, since such variables, especially in regard to southern senators, are often used merely as surrogates for ideology or constituency composition, factors we measure more directly with ADA scores and demographic characteristics.

4. As is usual in such models, determining statistical significance can be difficult and is open to some debate. We follow one standard practice, which is to interpret slope coefficients roughly as t-scores and evaluate them, in this case using a one-tailed test. The utility of such measures of significance is further complicated by the fact that the vote here is a population, not a sample, rendering the substantive interpretation more interesting than the statistical one.

5. In calculating the reduction-of-error (ROE) figure, which serves as a test of goodness of fit, we follow Lindsay 1991 and Hagle 1989:

$$\text{ROE}(\%) = 100 \times (\text{predicted } \% - \text{modal } \%)/(100 - \text{modal } \%).$$

6. In addition, not only is Clarence Thomas a native of Georgia (and, therefore, in a sense, a constituent), but Fowler depends heavily on the African–American community for reelection support. In his 1986 election, Fowler carried 75% of black votes, while winning only 39% of white votes (Petrocik 1987).

7. Moderate Democrats are, of course, the most interesting category in this regard. As "gettable" swing votes whose ideological predispositions had not already made their support virtually impossible, they seemed to be the focus of administration lobbying efforts in the days and weeks before the confirmation vote (Moore 1991).

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CONTROVERSY

GOING BEYOND THE STATE?

Timothy Mitchell's article "The Limits of the State" in the March 1991 issue of this Review *stimulated an unusual variety of interested comments. John Bendix, Bartholomew Sparrow, and Bertell Ollman offer critiques and suggestions from quite different points of view. In response, Mitchell clarifies further the distinctiveness of his own approach and its implications.*

COMMENT

The conceptualization of state and society remains troubling. We are dealing not with mutually exclusive categories but with a social and political relationship whose contours and definitions change over time and space. There is an unclear separation between the abstractions and the actual institutions said to be part of the state or society. We hold exaggerated, even romanticized, images of the total domination of state over society (absolutism, fascism) or the total domination of society over state (Marxism, anarchism). When we speak of the limits of the state we imply some point beyond which the state cannot go. Timothy Mitchell's "Limits of the State: Beyond Statist Approaches and Their Critics" (1991) has renewed this central debate for political science; and the neglect of the state that he attributes to the rise of behaviorism and systems approaches in the discipline echoes other critiques (Almond 1990; Caporaso 1989).

Mitchell argues that if we understood the state as "the powerful, metaphysical effect of practices" (p. 94) arising from internally erected distinctions in both state and society, rather than as an actual structure, we would come closer to the concept of the state. While this clever definitional solution owes something to Foucault, Mitchell's treatment of the "essentially idealist" approaches to the state taken by Nordlinger, Krasner, and Skocpol is disembodied, as though they, too, were not responding to previous concepts of the state. The historical development of state structures needs discussion if the "actual state structure" is not to become a straw man. Most importantly, Mitchell's contribution to the debate neglects how the state and society influence one another (Almond 1990).

The traffic across the border between state and society is more interesting than where the ultimately somewhat arbitrary demarcation line is drawn. Already in 1931, one observer claimed to have found 145 different definitions of the state (see Caporaso 1989, 28). States exist by virtue of, and together with, societies. The boundary is defined and contested; but *who* is delineating, negotiating, and crossing that boundary for what purpose is more important to investigate. The interactive elements complicate this pair of terms and make it interesting. To focus on the appearance of the boundaries imposes a definitional separation about which there is great disagreement. Recent analyses of interpenetration and mixed modes (corporatism, intermediate associations with both public and private functions, the interaction of bureaucracies with private interests) seems a more fruitful means to find the "limits of the state."

I suggest an expansion of Mitchell's argument by reviewing the state as concept, as law, and in its historical development. I suggest that we look at the state less in terms of its limits than in terms how it has been conceptualized and put into practice.

THE STATE AS CONCEPT

Max Weber's definition of the state as possessing the monopoly of the legitimate use of force is well known, as is his notion that the system of order of the state claimed binding authority over both citizens and territorial jurisdiction. Less well known is that Weber's state was based on the idea of a powerful corporate group whose membership either closed or limited outside access and whose functioning accorded with norms that corporate groups regarded as valid. While a person or persons were in authority to carry out the order that governed the corporate group, the state as an administrative and legal order was regulated and restricted by legislation (Weber 1947, 145, 156). Weber's definition was neither teleological nor idealistic; and he was aware that the combination of bureaucracy, legally based order, compulsory territorial jurisdiction and monopolization of the legitimate use of force rarely occurred (Bendix 1977).

Two major categories of objections to this definition of the state have been made. The first denies that the state is either unique (as legitimizer of associations, as claiming binding authority over its citizens, or as having a special life-long concern for its citizens) or omnipotent (Weldon 1960, 48–50). I am neither sanguine about the state nor convinced that the state is an association like other social groups. If one applies Hirshman's exit, voice, and loyalty typology to social groups, then state membership is of a rather different order than societal membership (Bendix, Bendix, and Furniss 1990; Skowronek 1982).

The second objection is that states act in class interests. Claus Offe's state sees the bureaucracy as the mediator of a class struggle in the capitalist accumulation process, Nicos Poulantzas' (and Antonio Gramsci's) state reflects the issues of the dominant class and becomes the "materialization and condensation of class relations" (Carnoy 1984, 107, 130, 254; Chilcote 1981, 366). Yet there are "serious (empirical) questions about the plausibility of a theory of state autonomy for any advanced capitalist country" (Domhoff 1990, 28).

But pluralist, elite, and Marxist views of the state may not be incompatible. Each theory equally permits its adherents to assert a *cipher* image, where the state is passive and ultimately controlled by civil society; a *guardian* image, where the state is an autonomous force that can balance out the social pressures being put on it; or a *partisan* image, where the state advances the interests of its constituent parts, whether organizational or individual, though under constraints and through bargains struck with other social groups (Dunleavy and O'Leary 1987, 327–31).

One cannot understand states without making reference to society, and our confusion about their interaction must be laid at the feet of Hegel. Hegel's universalist state is an impossibility of enlightened absolutist guardianship bound to personal independence anchored in an internalized Kantian sense of duty. The state is the evolutionary high point of the development of the universal; civil society is private, filled with individual interests and activities, and by its very nature cannot achieve the necessary universal freedom (Chilcote 1981, 189). Society incorporates certain universal qualities but has to be guarded by the state; for the two are mutually dependent, interfused, and interacting, though society is clearly subordinate to the state (Barker 1961, 48–50). Hegel's argument permits both state and civil society to be *relatively* autonomous from one another while still exercising reciprocal influence.

Marx's reformulation is intended to undermine what he saw as a "separation of state from civil society" in Hegel (Marx 1977, 33). Marx believes that class domination would lead to a society subservient to the state, which is unacceptable (Chilcote 1981, 421; Giddens 1985, 20–21). Marx's *Critique of the Gotha Program* states that "freedom consists in converting the state from an organ superimposed upon society into one completely subordinate to it" (1977, 564). Hegel had not acknowledged that "developed civil society" remained hampered by the fetters of the privilege system, Marx felt; and civil society was "political society because the organic principle of civil society was the principle of the state" (quoted in Chilcote 1981, 190).

Interpreting the state as subservient to society—in part a legacy of Marx and in part a legacy of legitimizing popular sovereignty—has created an interesting problem. To many observers today the state has no other role to play *but* to serve the citizenry. "If the state has a proper realm," Harry Eckstein wrote, "then it surely is the redistributive" (1979, 16–17). We are left with the illusion that the "state is hard to distinguish from the citizens in whose name . . . its authority is being exercised" (Watkins 1968, 152–53).

The state should be responsive to society, but total societal dominance would make it impossible to meet citizens' needs. No one would guarantee distributive justice, protect the health of citizens, secure national defense, or even provide an alternative to a massive private market (Hague and Harrop 1987, 20–22). The inadequacies of a weak civil society, by contrast, have been very evident in Eastern Europe in the last few years.

Weber's conceptualization is firmly anchored in his ideas of society. The state stands at some remove from, but us ultimately under the control of, social forces; and it cannot avoid interacting with society. Weber reformulates Hegel's state-dominant, universalist interpretation, while maintaining a good portion of the interactive character of civil society with the state. Such a view also emphasizes the importance of popular sovereignty—a notion not easily reconciled with Hegel's emphasis on the guardianship role of the state. Marxist and neo-Marxist visions, on the other hand, have tried hard to eliminate the dynamic tension between state and society. Marx's attempt to undermine an artificial separation of state from civil society is commendable (if a misreading of Hegel); but his solution of favoring society over state to right past wrongs is not. The answer cannot lie in replacing state dominance with societal dominance. The neo-Marxist reinterpretation of the state distorts even Marx's reinterpretation of Hegel. The interaction between state and society is not made clearer by treating the state as a cipher and the power of society as emerging from a single class. Dunleavy and O'Leary's (1987) partisan image at least allows the possibility that both state and society may have their interests, bargains, and constraints.

THE STATE AS LAW

A different answer has been provided by those who see the state as law or popular sovereignty. Modern sovereignty implies both an older supreme power (the monarch's) and a newer legitimation through institutions (legislature, cabinet, executive, citizenry) tied to a constitutionalism that marks state institutions off from society. Social groups are inferior to the sovereign state, yet sovereignty based on popular legitimation makes the state ultimately subservient to society.

Modern sovereignty implies legally sanctioned institutions. The state today is defined as a legal association organized under, and by virtue of, a constitution operating under a rule of law that guarantees the rights, and establishes the duties, of citizens. The state is the "sovereign safeguard" that secures an "external framework for the inward movement of a good life" (Barker 1945; Barker 1961, 47; Benn and Peters 1968, 296).

Government in contemporary democracies is formally accomplished in accordance with written constitutions and administrative procedures that rest upon a system of laws. Yet legal systems differ. In

Continental European Roman law a set of transmitted written codes depended for its authority "on the powers inherent in the person or persons promulgating it" (Hall 1989, 10). Code law implies a subservient judiciary (to emperors, legislatures, legal experts) and thus a state above society, guiding and regulating it and distinct from it. Anglo-American common law, with its independent judiciary and constitution legitimated from below, places society above the state, making law and government the product of the customs and habits of its people. Such legal differences affect how state or government and its relation to society are analyzed.

THE STATE AS THE PRODUCT OF UNIQUE HISTORICAL CIRCUMSTANCES

The Europe of 1500 had 500 political units, the Europe of 1900 only 25 (Tilly 1975, 15). The fragmentation of the medieval era—with its array of economic institutions (guilds, merchant leagues), ecclesiastical power (churches, monastic orders), secular centers (cities, universities) and hereditary rule over bits of territory (aristocrats), all struggling for dominance and autonomy (Eckstein 1979, 6)—allowed for a consolidation by particular hereditary leaders through force, fraud, and alliance. Initially, there was little distinction between the monarch's state, family, property, and society: the French king governed with a family council; Frederick William I of Prussia in 1740 earned as much from his royal domains as he gathered in taxes; and in England the law of property encompassed the rules about public office (Barker 1945, 4–6). Yet increasing central control diluted the monarch's power—Frederick the Great claimed he was only "the first servant of the state" (Dyson 1980, 28)—because a larger territory needed to be defended. Defense meant armies and the development of an efficient, nationwide system of administration to raise the money for the army through taxes. "The business of administration," Ernest Barker notes, "becomes the business of providing and equipping an army" (1945, 39).

The alternatives to paid mercenaries—universal conscription and reliance on local nobility for troops—involved political challenges to the monarch. The first implied functional equality through equal obligation, at odds with the social hierarchy of absolutism; the second led to demands by local notables for favors or representation in return for providing men or money to the king. Yet the king's need nevertheless ushered in the functional equality of treatment through the formally uniform application of obligations across territory and subjects by a national administration.

Society had to become an entity free of the king and the estates that supported him, to move away from its connotation as "high society" and toward a politicized sense of groups organized for collective action. The French Revolution ushered in the new category of citizens and, with it, a politicized society that legitimated self-selected institutions. To the centralized, administrative state was added the new nation and a more inclusive society. Yet the newly legitimated state used the model of central penetration and standardization pioneered by absolutism (Tilly 1975, 66). The rise of market economies had undermined the privileges of estate, and the extension of suffrage made political participation increasingly inclusive and oriented toward collective action. Society came to mean the articulation of political and economic interests from below, even while the economic imperatives of nation–states necessitated continued involvement in the economy from above, particularly in the state regulation of property, status, and money (Fried 1968). In this history one begins to see a sense of the state as a commonwealth, rather than a paternalistic fiefdom, though the vestiges of a sense of the state imbued with idealistic "ennobling sense of purpose" remain as one of the more benign legacies of absolutism (Dyson 1980, 3, 6).

Absolutist rule bore within it the seeds of its own destruction, yet institutional structures established under the aegis of monarchical rule were not entirely abandoned. The role of the state now to guarantee the well-being of the citizens, combined with changes within the market, as well as the rise of nation–states, gave the new institutions of government a role to play in the regulation of property and rights, as well as continuing the older roles of defense of territory and collection of money. The participatory and arbiter sense of state arose with the civil society of the French Revolution, with its functional representations of interest and its competition among organized societal groups (Benn and Peters 1968, 348).

CONCLUSION

I agree with Mitchell that analyzing the state as a free-standing entity is not helpful, but I do not agree that the solution is to treat the distinction as a matter of appearance or the reproduction of a "line of difference." It too readily turns what should be a dynamic interaction into an issue of postmodern semantics.

Understanding the state-and-society boundary better is akin to asserting that the crucial problem in Eastonian systems theory is understanding where "the political system" ends and "the environment" begins. If, as Mitchell implies, the conceptualization of politics in terms of a "system" is flawed, what have we gained by better understanding the external and internal boundaries of that system? American political science has found it easier to study the outputs of institutions than to dwell on the public purposes that motivate state actions and easier to examine welfare policy than the concept of what the general welfare of the citizenry might be. Other intellectual and legal traditions indicate that this need not be the only way to understand government, though it may be a distinctive feature of British pragmatists and American empiricists to analyze parts, rather than wholes (Dyson 1980, 17). However, organic or cybernetic models, with their use of the "political system" as a substitute for the state (with its inputs, throughputs, and outputs) may not be the most appropriate means to study how history, ideas, and law have interacted in the modern state.

Our conceptual problems come from replacing a static but interdependent Hegelian conception of state dominance and civil society subordination with a relatively dynamic Marxist idea of the development of societal dominance over a state compromised by class forces. The Weberian focus on the binding authority exercised by an exclusive corporate group gets lost in the shuffle. Neo-Marxist instrumentalist views only reinforce the dominance of society *over* state, rather than the interaction of society *with* state. The fusion of state and society provided by deconstructionist orientations does not aid our understanding of the relationship but merely comments on the relationship.

Boundaries do have meaning, whether in the empirical world of keeping undesirables out and citizens in, in the psychological and cultural sense proposed by Fredrik Barth, or in the abstract imperial claim to render unto Caesar that which is Caesar's. But boundaries are crossed, contested, opposed, and acquiesced in by those whom they affect. Boundaries are porous and changeable; and it is in the analysis of migrations across such boundaries that the state and society negotiate their relationship.

But we cannot escape the contradictory tradition of the European state. The modern state emerged from a history of absolutist guidance from above and overarching purpose, combined with a stratification that demarcated who had political and economic power and who did not. Its evolution into a benign guarantor of individual rights and policer of contracts makes us forget that it remains the authoritative definer of the public interest.

Jacob Burckhardt argues that it is an arrogance for the state to "attempt to fulfill moral purposes directly, for only society can and may do that"; the state is the guardian of the law and its business is to "prevent the various conceptions of civil life from coming to blows [so that the] various egoisms but also the various metaphysics shall carry on no blood feuds" (1943, 118–19). Burckhardt's state is custodial, protective, and based on law, rather than social contracts—a union based on conscious reciprocity (Dyson 1980, 11). This humane, functional characterization emphasizes what can be best about a state—not its limits or its definition but how it may interact with what is best in the society it is responsible for.

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COMMENT

Timothy Mitchell (1991) attributes to its elusive boundary the difficulty in defining the "state." One group of scholars, including Gabriel Almond and David Easton, finessed the problem of the state–society boundary by abandoning the state as a concept. But their "political systems" approach could not wholly avoid the issue of what constituted the "edges" of the political system—so the boundary question remained, as salient as ever. Other scholars, such as Eric Nordlinger, Stephen Krasner, and Theda Skocpol, resurrected the state as a subject of study but perpetuated this boundary ambiguity by (unhelpfully) restricting the state to a subjective arena of autonomous decision making.

Using the relationship between the U.S. government and the Arabian American Oil Company (Aramco) as an example, Mitchell argues that the state does not mark a "real exterior." Rather, the distinction between the state and society (i.e., among the institutions maintaining societal order) is *itself* a part of a "larger political order"—an order in which Aramco's "nonstateness" is also a resource. Mitchell finds the state–society boundary to be a "structural effect" that identifies which institutions constitute society and which, the state. Central in the creation of this structural effect is the production of *disciplines*, exemplified by the formation of the French Army as it became separate from society. Mitchell concludes that the production and reproduction of the definition between the state and society is "the essence of modern politics" (p. 95).

Mitchell's provocative thesis prompts a two-part response. The first part concerns the primacy of the boundary problem in the study of the state; the second part addresses Mitchell's alternative to a direct study of the state, the study of disciplines.

PROBLEMS WITH THE STATE

Since "a definition of the state always depends on distinguishing it from society," Mitchell holds that the "difficulty in drawing the boundaries of the state" is the fundamental issue for the state, explaining its controversial acceptance in American political science (p. 77). But to implicate the boundary problem as the cause of the "ill-defined" modern state ignores other, important causes for the state's imprecise definition. The presence of these additional reasons casts doubt on the centrality of the state's boundary-drawing problem.

One reason for an imprecisely defined state is the shadow that the distinct conditions of the United States cast on American and international scholarship on the state, granting the international impact of American political science. Whether for reasons of universal (white male) suffrage preceding industrialization, the lack of a feudal heritage, the self-selection of its inhabitants (with the important exceptions of

African Americans and Native Americans), the establishment of a federated and fractioned system of national government, or other factors, the United States has been less amenable to the state label compared to other states (especially relative to a Bismarckian Germany or an absolutist France). After all, Philippe Schmitter's corporatism, applied frequently to studies of European or Latin American governments, for instance, is scarcely used in research on the American government (Schmitter 1974, 1986). But an application of *the state* both to the United States and to other nations is explicit in the work of J. P. Nettl, Krasner, and Skocpol and implicit in the work of Easton, Nordlinger, and others. The different development of the American and non-American states ("American exceptionalism") has played a key role in the troubled concept of the state.[1]

A second reason for an ill-defined state comes from deficiencies in its specification, a problem that is more the consequence of method and less the product of the boundary issue. American political science, with its emphasis on positive research, has been not particularly receptive to the Marxian states of Fred Block (1977), Ralph Miliband (1969), Claus Offe (1974), or Nicos Poulantzas (1973), to name a few.[2] But the work of Krasner, Nordlinger, Skocpol, Stephen Skowronek, and others—work that succeeded the Marxian scholarship and that has been instrumental in bringing about the resurgence of interest in the American state—has conceptual and methodological ambiguities of its own. Three problems, in particular, attend this recent scholarship.

One is simply *defining* the state, as noted by Mitchell. Krasner defines the state as "the President and those bureaus relatively isolated from societal pressures" (1978, 62), a definition that allows for such an insulated state to take a variety of forms over time.[3] Nordlinger, on his part, defines the state as all public officials, federal, state, and local.[4] But Nordlinger's argument on the state's potential autonomy depends on state officials having subjective and independent preferences. Meanwhile, Skocpol's analysis of state autonomy includes state–society relations in its explanation, as Mitchell recognizes (pp. 87–88). Not only is there little agreement by those studying the state on what comprises the state, but those studying the state present problematic definitions of their own. The state has to be pinned down.

Second, there is the problem of determining the status of the state as an *explanatory variable* (see my earlier work, Sparrow 1991). Mitchell writes of the state exclusively as a dependent variable, for example, while Nordlinger considers the state solely as an independent variable.[5] Skocpol uses the state at once as an independent variable, an intervening variable, and a dependent variable.[6] But the status of the state as a variable is of obvious importance in assessing the validity of a state-centered explanation and in evaluating the salience of the state-society boundary. The explanatory role played by the state has to be made clear.

Then, there is a third problem—uncertain *explanation of change* in the state. Change in Krasner's state comes about through an indeterminate convergence of political forces (president, Congress and congressional committees, federal bureaucracies, pressure groups, and even public opinion) on particular policies (1978, 61–66).[7] Skocpol, on her part, does not present a consistent theory of state building (Burawoy 1989).[8] Neither does Mitchell provide the reader with the mechanisms of state change save for a discussion of disciplines (which I take up later). In short, the mechanisms of state change—taking the state as a dependent variable here—are often inexact.

These problems of specifying the mechanisms of change in the state, revealing the status of the state as a variable, and offering precise and consistent definitions of the state raise serious questions for scholarship of the state. These questions are separate, for analytic purposes, from those that address the state–boundary issue directly. The problems in conceptualizing the state warrant separate consideration. Likewise, the problematic application of the state to the American polity and to other (particularly non-Western) polities is an issue that merits serious attention in its own right.

INDETERMINATE DISCIPLINES

The other part of my response to Mitchell concerns his implications for studying the state and the state–society boundary. Mitchell's conclusions are that (1) the state should not be seen as distinct from society, (2) the state-society boundary should be the point of focus in the study of modern political order, (3) the study of the state as the locus of decision making and the policy process cannot suffice, (4) the state should be addressed as the effect of detailed processes that divide the state from society, and (5) the separate structures of the state should be recognized for their abstracted and constructed nature. The crux of Mitchell's project centers on the last two points, however—that the processes dividing the state from society should be seen as part and parcel of the use of power and that perceptions of such processes are themselves resources for political control. It is Mitchell's ambition to reveal that state and society are constructions and that existing institutions contain hidden imbalances of power. Yet this deconstructionist project amounts to an indeterminate and ultimately unfalsifiable political science.

Consider Mitchell's language: "The appearance that state and society are separate things is part of the way a given financial and economic order *is maintained*" and "The *arrangements that produce* the apparent separateness of the state create the abstract effect of agency, with concrete consequences. Yet such agency will always be contigent upon *the production of difference*—upon those practices that create the apparent boundary between state and society; his ultimate object of study, "disciplinary power," operates "not from the outside but from within, not at the level of an entire society but at the level of detail, and not by constraining individuals and their actions but *by producing them*" (pp. 90–91, 93; emphases mine). The study of power-wielding, boundary-making institutions is typified by the absence of agents acting on behalf of any dominant interests and the omission of mechanisms connecting individual and organizational behavior to systemic outcomes.

The reader is left with little notion of the particulars of such discipline production, however. Take Mitchell's example of the creation of the French Army: "Of course, this apparatus [the French army] has no independent existence. It is an effect produced by the organized partitioning of space, the regular distribution of bodies, exact timing, the coordination of movement, the combining of elements, and endless repetition" (pp. 93–94). Space *is partitioned*, bodies *are distributed*, timing *is made exact*, movements *are coordinated*, elements *are combined* and effects *are repeated*, all to the perpetuation of existing power relationships. This is nothing more than systemic functionalism: "State *practices* define and help constitute a national entity . . . by establishing a territorial boundary and exercising absolute control over movement across it" (p. 94; emphasis mine). Mitchell nowhere provides the reader with the subjects who produce difference or maintain order.

The use of the term *disciplines* follows from Michel Foucault, of course. But Foucault's (1987) *Discipline and Punish* reveals the same agentless explanation of the penal system: "And, if one can speak of justice, it is not only because the law itself or the way of applying it serves the interest of a class, it is also because the differential administration of illegalities through the mediation of penalty forms part of those mechanisms of domination" (p. 272). Law, or its application, serves class interests; and the differential enforcement of the law is part of the same class dominance. Foucault continues, "Deliquency, solidified by a penal system centred upon the prison, thus represents a diversion of illegality for the illicit circuits of profit and power of the dominant class" (p. 280). As Foucault summarizes the penal system, "A whole series of illegalities *was inscribed* in struggles in which those struggling knew that they were confronting both the law and the class that had imposed it" (p. 274; emphasis mine). The outcome (maintained class division) explains individual and organizational behaviors (delinquency and the particular characteristics of the penal system). Again, this is the logic of systemic functionalism.

Raymond Boudon observes that Foucault ends up positing that the ruling class has absolute power, can conceal this power, and can at once exploit the penal system to its own advantage while criticizing imprisonment. As Boudon puts it, "Prison is useful to the police, and the police are useful to the ruling class" (1987, 136–37). Mitchell appears to take this same position: "The appearance that the state and society are separate things is part of the way a given financial and economic order is maintained" (p. 90). The creation of disciplines allows the state to act, directly and indirectly, in the interest of existing power relationships. Only we are not told how to study the detailed processes that actually produce the state–society boundary.

Yet there exists a body scholarship that could serve as a point of departure for a detailed study of institutional behavior and social processes—the work on organizations and institutions coming out of the Carnegie school (e.g., Simon 1948, 1957; March and Simon 1958; Simon 1962; and Cyert and March 1963—and, more recently, March and Olson 1984 and Moe 1984).[9] "Neoinstitutionalists" share a focus on individuals precisely *not* for establishing an independent "set of intentions or preferences, just as persons are thought of as units of autonomous consciousness and desire distinct from their material or social world," as Mitchell has it (p. 88) but, rather, for establishing the cognitive bases of organizational and institutional action.[10] This new institutionalism also pays heavy attention to an organization's environment, acknowledges the loose coupling among organizational components, and emphasizes the reflexive aspect of individual behavior (Powell and DiMaggio 1991, 11–19). By examining the embedded and uncertain qualities of individual action, such an approach is able to provide the microfoundations of organizational and institutional behavior.

Not only is organizational and institutional behavior the product of individual action, but the reverse holds, as well: standard operating procedures, organizational routines, classifications, and framing may determine individual actions and, hence, aggregate outcomes. Once habits, roles and labels become established and taken for granted (even if themselves human creations), they may reciprocally affect individual actions. The preferences of corporate, associational, and governmental actors are not developed autonomously or in distinction to material or social context but are *learned*.

BEYOND THE STATE?

The state may be defined inexactly for reasons apart from its boundary problem. One reason is that the distinct qualities of American political institutions, less amenable to the state label for reasons indigenous to the United States, has hindered the specification of the state in American political and social science. Another reason for the troubled concept of the state has been the methodological imprecision of scholarship on the state, imprecision that has taken at least three forms: (1) those researching the state offer problematic definitions of it and thereby obscure its conceptualization; (2) scholarship on the state has often been inconsistent or ambiguous on the status of the state as a variable; and (3) an inattention to the mechanisms of state change has made the state difficult to study. It is by no means clear that the state–society boundary is the primary difficulty in defining the state or that the troubled boundary between the state and society in fact defines the modern political order.

We might be cautious before going "beyond statist approaches." A focus on disciplines provides little assurance that research on the state will be other than nonfalsifiable or externally invalid. Neither is Mitchell's attention to microprocedures (the processes that create the institutions constituitive of the state and society) novel. And the recourse to functionalist explanation may be a step backward for political inquiry. When Mitchell says that the state "should be addressed as an effect of detailed processes of spatial organization, temporal arrangement, functional specification, and supervision and surveillance, which create the ap[p]earance of a world fundamentally divided into state and society" (p. 95), we may wonder (1) *to whom* the state and society appear so fundamentally divided, (2) what is *new* about turning scholarly attention to organizational configurations and microprocesses, and (3) *who or what* are effecting such divisions between the state and society through the creation and nurturing of these discipline processes.

In consideration of these remarks and in view of Mitchell's own telling arguments—arguments that debunk the attributed coherence of the state and that explain that the boundary of the state does not actually separate between the mechanisms facilitating social and political order—what may we conclude about the study of the state?

CONCLUSION

It may be hard to do better than recommend a return to the work of Max Weber. Weber defines the state as "an administrative and legal order" with "binding authority" over "the area of its jurisdiction." For Weber, the state "is thus a compulsory association with a territorial basis" (1978, 56). A Weberian definition of the state would identify the state with legal–coercive bureaucracy. But Weber also remarks upon the *zweckrational* behavior of modern business and public administration. The study of the codification, standardization, centralization, and specification of authority in governmental administration—an interest shared by Mitchell—is also included in Weber's analysis (1946, 196–244). Statist research would examine these instruments of administration and policymaking.

But to study the bureaucracy and the means of administration by themselves would still be to restrict the study of the state; for if we are to study the state as a political association claiming a monopoly of legitimate violence (Weber 1978, 54), we need to consider the institutions exercising a monopoly of authority, whether autonomously or through the joint operation of governmental and societal actors. Recall that Weber places primary emphasis on the explicit or implicit coercion of the state (pp. 903, 909). In the terms of Nettl's four ways of defining "stateness," it may be possible for the state to be an institutionalized collectivity (dimension 1), and international unit (dimension 2), and a sociocultural phenomenon (dimension 4) but not have sectoral autonomy (dimension 3). Nettl (1968) omits a consideration of coercion (or nonvoluntarism) in discussing the state, even though he cites Weber in multiple instances. Similarly, Mitchell mentions coercion once (when discussing power and the institutions creating and reproducing power, p. 82) but does not touch on the *monopoly* of power.

The monopoly of power is crucial for any definition of the state, however; for the presence of such a monopoly may implicate nominally private actors, such as professional associations, business corporations, labor unions, and even individuals, in the state. Consider the government–societal ties implicit in extensive transactions in the receipt of retirement benefits and government pensions, the sale of government securities, the payment of income taxes, and military spending. Indeed, Mitchell's reference to investigating "the relationship between state and private institutions in the financial sector, in schooling and scientific research, or in health care and medical practice" suggests precisely this focus (p. 90). This focus is explicit in the work of Laumann and Knoke (1987), who define the state as the networks of interaction between governmental actors (congressional committees, federal bureaus and agencies, and the White House office) and societal actors (trade associations, business corporations, labor unions, and other pressure groups). It is implicit in the work of Skocpol (1979, 1981, 1985) and Ikenberry (1988), who look at state–society relations as part of a statist analysis but do not include such relations as part of the state. The state–society boundary *is* problematic; but it may not be so much a "boundary" as a sector of coordinated, and possibly conflictual, government–society interaction that exercises degrees of jurisdictional monopoly.

A *fourth* response to the problem of the state may be added to Mitchell's three. Besides eliminating the state from study (Almond, Easton); confining it to an autonomous existence (Nordlinger, Krasner, Skocpol); or studying it as a structural effect (Mitchell), we could expand its definition to include the societal actors that—either in direct conjuction with, or tacit agreement with, governmental actors—are able to exercise effective monopolies of authority. Indeed, the literature on "policy networks" and "iron triangles" addresses this very overlay of the "public" and the "private." To study the state defined along this line demands a respecification of the state, however, rather than its relegation to the realm of a "metaphysical effect."

It may be true as Mitchell says, that Weber's definition of the state "does not tell us how the actual contours of this amorphous [state] are to be drawn" (p. 82); but it is also true that Weber never finished his analysis of the state (Bendix 1977). Studies of the state, just like those of the frontier, the law, or army building (subjects suggested by Mitchell) no doubt *can be* specified. Such a program for examining bureaucracies, the means of policy administration, and the relations between the government and society calls for innovative research able to provide indicators of the multiple dimensions of national power—the composite, fractioned, and aggregate qualities of a state—and to test propositions on these constructions of national power. It is incumbent on students of national political authority to do so—to attempt to update Weber's work on the state.

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COMMENT

To his credit, Timothy Mitchell (1991) does indeed go "beyond" the boundaries that have been drawn in most debates among political scientists on this subject, but he leaves us with the question whether he has gone far enough. After effectively criticizing various writers' attempts to establish a clear division between state and society, Mitchell offers a version of their union that emphasizes people's numerous everyday activities that constitute the forms and hierarchies of order and control (our political institutions), while simultaneously making these institutions appear separate from, and independent of, the larger social context. In this way, the same practices that give rise to the state are held to be primarily responsible for the widespread misunderstanding of its nature—in particular, its limits.

But why do we want to understand what the state is in the first place? Primarily, I would have thought, because we want to understand better what the state does. This suggests that most people already have a rough idea what the state does: it makes laws, administers and enforces them, adjudicates disputes on the basis of such laws, and represents people who live under the rule of one state in their dealings with other states. Until greater clarity is attained regarding who and/or what does all this, however, it is difficult to distinguish the state from such kindred phenomena as the government and the nation or to understand why setting the boundaries of the state should be such a problem. Worse, it is impossible to explain the patterns observed in the state's conduct of its work and the effects resulting from there, namely, that some groups in the country consistently play a greater role in the higher reaches of the state and benefit to an inordinate degree from most of what the state does. Well, who are they, and why should this be so? And does it have anything to do with the nature of the state as such? These would seem to be reasonable, and even obvious, questions.

While all the writers that Mitchell criticizes are indeed guilty of setting up the state as an independent entity, they do at least provide us with answers to these questions if only to deny the accumulated evidence—though it is usually more than that, like attributing the observed bias to the decisions of politicians. Mitchell, on the other hand, offers no answers whatsoever. The Foucault-inspired micro practises of "organization and articulation" (p. 92) that constitute the state in his account are disembodied, depoliticized, declassed, and, despite a couple of vague references to history, ahistorical. Emptied of all social content, they can tell us nothing about why the state acts as it does.

Yet there is much in Mitchell's essay that suggests he really knows what the answers are. He says, for example, that the spillover of society into what is ordinarily taken to be the state, their internal relation, has to do with how a "social and political order is maintained" (p. 90). But we never learn who benefits most from maintaining the present social order or how, exactly, the state helps them. The social order itself is never named. Capitalism is nowhere mentioned in his article.

Further, Mitchell is aware that a major effect of the attempt to remove the state from society is that it reinforces the view that the state and/or officials acting on its behalf are free and independent agents, only occasionally influenced—and then only to a minor degree—by larger social forces. From this, it is but a short step to recognize (as I believe Mitchell must, although he nowhere makes it explicit) that there are few beliefs more important to the health of the status quo than the one that treats state institutions as neutral in the political and economic clashes between different groups, implying that each of them has a more or less equal chance of winning the support of these institutions. To believe that the state has special ties to one group, ensuring that its major interests always come on top, would cost the state a great deal of its legitimacy and make it far more difficult for it to do whatever it does on behalf of the favored group.

Finally, Mitchell's own examples suggest to us that he knows more than he says. Respecting the division between state and society, he says, "The essence of politics is not politics formed on one side of this division being applied to or shaped by the other, but the production and reproduction of this line of difference" (p. 95). Do the Aramco Oil Company and American banks, two of Mitchell's main examples (pp. 89–90), engage in politics to establish a clear separation between the state and society, or to promote their own money-making interests? The question should answer itself. If, in the process of promoting their interests, they make use of means that disguise their integration into the state—chiefly as a way of avoiding democratic accountability—this certainly deserves to be mentioned; but it should not require that we exchange one disguise for another. While Mitchell isolates a major ideological distortion that emerges from the particular means by which a favored group helps constitute the state in the very process of using it, he does so in a manner that further mystifies who, exactly, is acting and why they are acting in just these ways.

Why such hesitancy? After criticizing various thinkers for setting the state out from society, Mitchell seems unwilling to investigate the systematic bias that he clearly suspects is present in state practices because he fears falling into the same trap as those he criticizes. To present the state as consistently acting as it does due to pressures (whatever their form) located in society would be to reestablish, he seems to believe, the very separation between state and society that he took such pains to demolish. In short, Mitchell's explanatory framework and commitments keep him from recognizing what, at some other level, he knows to be there.

Is there another way of treating this matter that would avoid this dilemma? Mitchell's own approach suggests that there is. The key to his approach involves extending the notion of the state back in time to include the wide variety of practices by which the institutions of the state came into being. Having stretched the boundaries of the state so far, there can be no objection, in principle, to stretching them a little further in order to include who and/or what are primarily responsible for these practices. Mitchell's conception of the state, in other words, could—and, in my view, should—include the group or groups whose special ties to the state give its forms and practices their distinctive character. This would enable us to explain the patterns we observe in the effects of its forms and practices without introducing a split between state and society. If the everyday activities of citizens helps explain how the state is constituted, it is only the connection between these activities and the requirements of the favored group or groups that can account for why it is constituted in just these ways at just this time. An adequate conception of the state must contain answers to the questions *why* and *when*, as well as to the question *how*.

There is no single answer to where the state ends and where society begins, because these are not neighborhoods separated by broad avenues. Rather, the one performs a range of essential functions within the other. The ways, and even the forms, through which these functions are performed, however, vary considerably depending on both the nature and the stage of development of the society in question. They also vary depending on which group has enough power based on its structural position in society to ensure that its interests generally emerge on top and that the requirements rooted in these special interests get taken as society's requirements. Such a group is more accurately referred to as the "ruling class." With changes in society, in its ruling class, and in what this class requires in order best to serve its interests, the functions of the state will also vary and, along with them, the boundaries that establish state limits, as well as the very meaning of the concept "state" in our discourse.

Functions imply a system, however; and our quest is complicated by the fact that the functions performed by the state belong to three different, though overlapping, systems. These systems are class society, capitalism, and modern American democratic capitalism. They differ from each other in the degree of generality that mark all their qualities—which is to say, our society is a class-divided society and, as such, possesses a host of interacting qualities that are similar to those found in other class-divided societies during the whole period that they have existed on earth (roughly 5,000 to 10,000 years). Ours is also a capitalist society and has many systemic qualities distinguishing it as such, which it shares with all the capitalist societies in the five to six hundred years that this social formation has existed. Finally, our society is a stage of capitalism that has come to the fore under the special conditions that have developed in America in recent years (say, the last twenty to fifty years). What we call "our society," then, is a layered construction composed of sets of internally related qualities that fall on these three different levels of generality, indicative of three different time frameworks.

While the qualities that belong to these three systems are often treated together, we can also distinguish between them; and for certain purposes, it is essential that we do so. As regards the state, it is clear that what is meant by the ruling class, as well as by its requirements, varies somewhat as we move from viewing the United States as a class society, to viewing it as a capitalist society, to viewing it as modern American democratic capitalism.

At the level of class society, the dominant economic class requires political help in reproducing the conditions that underlie its position of dominance. Such help involves various means of socializing the population (especially its most oppressed sections) into an acceptance of the status quo and a repressive apparatus (chiefly, police, soldiers, and courts) that can be called upon when the former do not wholly work. Socialization is aimed primarily at gaining popular acceptance for the institutions of private property, for the basis (whether by divine will, birth, or money) of the prevailing division of labor, and for the idea that the special interests of the dominant class are the same as the interests of the nation. The mystification involved in the latter leads Marx to refer to the state during the entire period of class history as an "illusory community" (1942, 74). It is a "community" because it tries to reproduce the conditions underlying the mutual dependence of all people living in a given geographical area; it is "illusory" because it privileges the interests of one class under the guise of serving the interests of everyone. So much is true of the state in all class societies.

But (as we said) ours is also a capitalist class society; and, as such, it requires political help tailored to the peculiar character and interests of the ruling capitalist class. Perhaps the most distinctive feature of the capitalists as a ruling class is their contant drive to accumulate, to treat wealth primarily as a means to produce still more wealth. While socialization and repression remain essential state functions for capitalism, the nature of accumulation in this period requires certain modifications in the forms through which these functions are carried out. To cite just one example, workers perform their roles in accumulation more effectively if they feel they have had some freedom in choosing jobs and at least the possibility of influencing state policy in so far as it affects their jobs. All of this gives to the production of capitalist ideology—and to the institutions devoted to this activity—a much more important place in capitalism (and therefore in the capitalist state) than it has in other class societies and their states.

The centrality of accumulation in capitalism also forces the state to undertake a function that does not exist in other class societies and that is helping the capitalists realize the value of their products. Given how capitalism works, capitalists simply require more direct political help in their efforts to enrich themselves than did feudal aristocrats or the slaveowners of antiquity. They require what amounts to state planning on their behalf—and they get it. While rendered mostly invisible by their ideology, such planning covers everything from investing, to borrowing, to keeping production costs down, to buying, to selling the finished products. Most of our tax, trade, monetary, and labor policies (including both laws and their administration) come under the rubric of state planning in the realization of value. The political institutions and practices that serve capitalist interests in these ways are essential parts of the capitalist state.

While Mitchell's examples show corporations and banks only interested in making money (an interest aided by "appearing" private, outside the state), he never examines the system in which this occurs. Consequently, there is no mention of capital accumulation and the kind of help this requires from the state. Had he placed his analysis squarely within a capitalist framework, he would have noticed that appearing separate from society is but one of the ways (though perhaps, ideologically, the most important) in which the state serves the interests of the capitalist ruling class.

Just as ours is a class society and a capitalist society, it is also (as I have said) the modern American democratic capitalist society, and some of what our state is and does pertains to the special interests of the American capitalist class, given its place and role in world capitalism at this stage of its development. If the first set of qualities we examined are shared with all societies within the period of class history and the second with other capitalist societies for the entire period that they have existed, these qualities apply only to the American democratic capitalist state of recent times. Among the new developments that have had a major impact on the character and practices of the state on this level of generality, three seem to stand out. First, the enormous increase in the gross national product, together with the intensification of competition between capitalist societies in the last few decades, has led to the state's taking on a larger and more direct role in the realization of value while, for good ideological reasons, insisting ever more loudly that nothing of the kind is happening. Second, the Cold War has given us the national security state, including the "military-industrial complex," which has greatly facilitated the capitalists' effort to repress internal opposition and bolster their profits by draping both with the mantle of national security. Finally, the development of communications technology has made it possible for the American ruling class to extend the political work of socialization in capitalist ideas and values to the entire globe, transforming the media into an indispensable arm of the state—which is to say, into an aspect of the state itself (or what Louis Althusser dubbed an "ideological state apparatus" [1976, 67–126]).

As our American capitalist class becomes increasingly international, with more foreign capitalists investing in the United States and more U.S. capitalists investing abroad, the political help required to realize value has led to the development of world institutions such as the International Monetary Fund, World Bank, and the General Agreement on Tariffs and Trade, whose economic labels disguise their important political functions. They are the beginnings of a world capitalist state, whose origins and development (as with the rise of the national states that occurred earlier) are best understood as political responses to the changing requirements of the ruling capitalist class on the level of generality of modern capitalist society. The national state in the United States is also losing some of its power to regional political forms, such as the New York Port Authority and the Appalachian Regional Commission, and for similar reasons: the interests of the capitalist class in this period are better served by having certain political functions performed in this way and on this scale. With no democratic accountability to worry about, the technical intelligentsia operating in these arenas can play the role of farseeing statesmen for the capitalist class as a whole.

Barring a socialist transformation, what we also have here is a preview of the next stage of the "American" capitalist state, with the national forms of the state being reduced to performing mainly repressive and socializing functions, while most of the help rendered to capitalists in their drive to accumulate and maximize profits is provided at the world and regional state levels. If we were describing a European country, we would have to add still another level, the multinational level of the European Community, which does not yet exist in the case of the United States but may soon, as the new economic treaties with Canada and Mexico begin to take on a life of their own. How all this will affect the national state's ability to perform its repressive and socializing functions remains to be seen.

Viewing political institutions and practices as internally related to the ruling economic class, as equal and necessary aspects of a more broadly conceived notion of the state, and as falling on three different levels of generality does not keep us from recognizing occasions when a set of unusual circumstances allows some government officials, political parties, and even whole institutions to operate in relative autonomy from ruling-class imperatives. The point is that this is the exception (and a relative exception at that, meaning short-term and for limited purposes) and not the rule, as is the case in most non-Marxist interpretations of the state. Such exceptions are also largely explainable in terms of the special circumstances that apply. These exceptions also become increasingly rare as we move from the more specific level of modern American capitalism, to the more general one of capitalism, to the still more general one of class society—which is to say that it was easier to have a New Deal (the part of it that favored the interests of workers) than it is to interfere with the structures that underly capitalist—and, more generally, ruling-class—domination.

Nor do I mean wholly to dismiss the struggle by subaltern classes, taking advantage of the problems and contradictions that come with the territory, to use some part of the state to promote their interests. While the cards are carefully stacked against such efforts, it is sometimes possible to win a little or temporarily or to keep a capitalist initiative from succeeding as much as it otherwise would. And by reordering power relations even slightly, every victory resets the odds for the major confrontations up ahead, when the problems inherent in the normal functioning of the system have reached the explosion point. The larger goal of these confrontations can only be to socialize the means of production and in this way deprive the capitalist ruling class of the basis of its power. Should the effort succeed, a new ruling class, representing now the great majority, would organize the state in a way that would best serve its special interests, once again altering the boundaries of the state in the process. The internal relations between state and society (a very changed society, hence a very different state) would remain the same.

If many political scientists treat the state as a kind of "ghost in the machine" (David Easton's not altogether friendly observation, quoted by Mitchell, p. 91), it is only because the machine acts in ways that belie all attempts at explanation that are restricted to the decisions of the people who sit at the controls. Something more and different has to be introduced to help account for the persistent patterns found in how the machine works. Until this is done, all attempts to kick the state out the door (e.g., in the manner of the political systems theorists) will only result in its climbing back in through the window. Recognizing this, Mitchell seeks to highlight the numerous everyday practices that constitute the state in its present form and enable it to function as it does. In so far as these practices are themselves left unaccounted for, however, he has just substituted one ghost for another. It is only by extending the connections that Mitchell *started* to trace back further, in order to take account of the political requirements of our ruling class—grasped as a ruling class in general, as a capitalist ruling class, and as the modern American capitalist ruling class—that the otherwise inexplicable movements of Easton's ghost (and Mitchell's) become comprehensible.[11]

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RESPONSE

In "The Limits of the State" I argued that the state should be approached not as an object, site, or agent, but as a set of powerful yet elusive methods of ordering and representing social practice. These methods create the effect of an agent or structure—the state—that stands outside the social. Most theories of the state respond to this phenomenon in one of two ways. One response is to mistake the effect for something real and reify the state as a self-contained entity. Without grasping the discursive strategies that generate the state effect, such responses merely add to existing representations and become a part of the effect. The other response is to dismiss the effect as illusory and turn attention to the real political forces that stand behind. This overlooks the power and complexity of a discursive order and tries to substitute a simpler, less ambiguous, nondiscursive reality. These alternative responses are illustrated by the two different kinds of reaction to my article published here, Bendix and Sparrow on one side and Ollman on the other.

I first argued that political science had always failed to reach an adequate definition of the state, one that can distinguish it from the social world it seems to order. This is because the boundary between the state and society is elusive, porous, and mobile. I suggested that this elusiveness should not be eliminated by sharper definitions but explored as a clue to the nature of the state. Rather than assuming the existence of a distinct object, we need to bring to light the everyday techniques of difference that create its apparent distinctness.

Sparrow responds with the traditional view that the elusiveness of the state is more a question of "conceptual and methodological ambiguities." He imagines these problems of method to be a separate issue from the discursive ambiguity of the state and offers three solutions:

First, Sparrow says, we must find a definition to get the state "pinned down." This misses the point. A set of practices whose effectiveness lies in making what is elusive, incoherent, and open-ended appear as an object that is ordered, bounded, and univocal cannot be pinned with a singular definition. A definition that is "precise and consistent," such as Sparrow demands, will not capture political relationships whose very nature is to be imprecise and inconsistent.

Second, is the state in relation to society an independent, dependent, or intervening variable? Sparrow claims that I present the state exclusively as a dependent variable. I do no such thing. The question presumes what my argument denies, namely, that state and society are separable units or spaces. It assumes that each unit has an essence independent of the other and that their relationship can be imagined in the language of nineteenth-century mechanics, in which cause-and-effect is a unidirectional movement between self-contained, externally related objects.

This problem is also found in slightly different terms in Bendix's critique, which faults my article for neglecting "how the state and society influence one another" and complains that I am advocating "the fusion of state and society." He argues, instead, for a study of the dynamic interaction through which "the state and society negotiate their relationship." As with Sparrow, Bendix's argument conceives these constructions of language, cultural representation, and organizing strategy not as aspects of discursive practice but as independently existing objects that enter into relationship with one another. To criticize, as I do, the image of state and society as separate objects is not to advocate their "fusion" into a single object. Rather, it is to propose a less mechanistic and more discursive understanding of how social reality occurs—that is, an understanding that takes the constructed nature of the social seriously, rather than relegating such construction to some secondary realm labeled "culture" or "belief," and that acknowledges that social constructions are not bounded entities with singular identities but strategies and relations that often exceed their limits, become displaced, reverse themselves, or otherwise elude the descriptive realism that sees them simply as objects.

Third, Sparrow imagines the mechanical relationship of state and society to form a structure fixed in place, rather than a process that remains fluid, emergent, and incomplete. He says that we must decide how change affects the state, assuming change to be an exterior force visted upon an otherwise stationary object. All three of Sparrow's solutions to the problem of ambiguity start from the wrong place and end up reproducing the views that my article critiqued.

The second part of Sparrow's response, still picturing the state as a machine, complains that I have not named the operators that run it. He wants me to identify "the subjects who produce difference or maintain order." Why assume that every explanation must lead back to agents who stand outside the machinery and produce or maintain it? Why must our theories recreate the impression that subjects produce difference, never difference, subjects? Sparrow later on contradicts his own criticism by admitting that "procedures, organizational routines, classifications, and framing may determine individual actions." Yet for Sparrow, this relationship must be imagined as reciprocal (the machines running the people who run them), because he works within the dualism that takes the world to be divided into institutions versus individuals, structures versus actions, system versus behavior, and state versus society. He never asks how or why this dualism has come to be so taken for granted. My article attempted to bring these assumptions into view by suggesting that if we are to understand the state, we must see it as an aspect of the history of this dualism. The neoinstitutionalists Sparrow urges us to read cannot help with this.

Nor can Weber. His definition of the state as an organization that successfully claims to monopolize the legitimate use of coercion will not do. As I have argued elsewhere, it is problematic to approach the question of political order in terms of the Weberian distinction between coercive forms of power and the noncoercive, or between force and consent (Mitchell 1990). Too many aspects of the political order cannot be made to fit the terms of the distinction—such as forms of disciplinary, spatial, or institutional power that are physical yet not based on force or forms of violence that create frames of meaning for the language of consent. Weber himself seems to have recognized the difficulties in applying his definition to specific cases. His own writing on the state tends to leave the definition aside and focus, instead, on the professional organization to which it refers. Bendix rightly draws attention to Weber's extensive and important writings on bureaucracy. But this work represents not so much a part of the broader conceptualization of state power that Bendix describes as a narrower, institutionally focused substitute for a definition that Weber himself was unable to make work.

The other kind of response to the elusiveness of the state is to see the state as a fiction, whose apparent power and autonomy disguise the real power of the ruling class that controls it. Ollman proposes that we eliminate the fictional distinction between state and ruling class by stretching the loose boundaries of the state to include the ruling class within it. This would improve upon the prevailing view of the state as an autonomous entity (as my examples of the relationship between state and capital illustrate). But in rejecting the view of the state and the dominant social class as objects or agents independent of one another, one does not have to amalgamate them into a single entity. Ollman seems to acknowledge this by consistently distinguishing between the ruling class as the sovereign agent that rules and the state as simultaneously the instrument and the disguise for the agent's rule.

Yet Ollman's response leaves open some large questions. Nothing he says explains how a group so small as the ruling class can "use" an apparatus so large and amorphous as the modern state to serve so successfully its requirements. Nor does he suggest an adequate account of how this success is so consistently disguised. If the state is the "illusory community," then we need a theory of how such an artificial totality is produced. How do the partial, particular, localized practices of state power come to appear something universal, general and permanent? I argued that the state characteristically appears as a structure, in the specific sense of an abstract, enduring, architectonic frame. How can one explain this effect of structure? Marx, for all his insight into the process of abstraction that underlies the fetishism of the commodity and the very phenomenon of capital, does not elaborate a theory of structure. He makes frequent use of the analogies of architecture and the machine; but to capture the modern illusion of a structural totality, he tends to fall back on the classical, premodern distinction between essence and phenomenon (cf. Althusser 1970, 184–93).

Offering no analysis of the methods of this illusion, Ollman runs the risk of overstating its effectiveness. His discription of the way the state is used to serve the processes of capital accumulation sounds like a hymn to capitalist efficiency. He admits that there may be exceptions to this ordered functioning of the power of capital but only under "unusual circumstances." The setbacks capital might suffer are never more than small or temporary. Capitalism is thus imagined in its own metaphors, as a complete and unified system whose energies are rationed and whose logic operates without flaw. Fissures are secondary, division is external and short-lived—until some future confrontation that destroys the entire machine. Why not imagine capitalism differently? Why not bring its incoherences and fissures to the center and see the state not as the tool of its efficiency but as a temporary and uncertain coherence somehow engineered?

By subordinating the state to the logic of capital accumulation, Ollman also implies that this accumulation process possesses a necessary and singular calculus. Yet it is far from clear that the variety of forms taken by state practice at different moments and in different places can all be deduced from the logic of accumulation. The work of the regulation school (see Boyer 1990), for example, has been exploring the recent history of different regimes of accumulation and opening up the complexity and variability of the relationship between state practice and methods of accumulation.

Ollman seems to be moving in an opposite direction, reducing five or ten thousand years of global history to just three levels of analysis. Yet even to use a single concept, the state, to cover such a range of historical experience seems liable to mystify as much as it illuminates. Is it appropriate to refer to a five-thousand-year-old requirement of "socializing the population" when even the concept of "the population" has a history of only two or three hundred years and relates to practices of a similar age? Is it appropriate to make the same historical reference to a "repressive apparatus" of police, soldiers, and courts when almost everything we mean by these institutions is equally recent in appearance? Ollman's second level of generalization, "capitalist" societies of the last five to six hundred years, also fails to correspond to the rise of these modern institutions. The innovation that Ollman associates with this time frame, state planning, is even more recent. Finally, Ollman's third level of analysis, American capitalist society of the last 20 to 50 years, corresponds neither to the origins nor to the decline of arguably the most distinctive feature of twentieth-century American capitalism, the Fordist regime of accumulation.

Although I agree with Ollman that it can be helpful to distinguish different levels of generalization and analysis, I find little correspondence between the logical levels he has chosen and the aspects of the state that he wishes to derive from each logic. Even if his three levels were differently or more accurately defined, moreover, it is not clear to what extent they represent separate "systems." At what moment in the last five or six hundred years, for example, and at which sites, does capitalism distinguish itself as a "system" from other systems around it? And why are there only three levels? Could we not discern additional, intermediate levels of analysis and numerous other systems and logics? Even then, might we not find that at each level and for each time frame state practice did not follow a unique pattern but developed a variety of forms and strategies—strategies that on occasion reverse themselves, fail, produce accidental side effects, or otherwise escape the kind of singular and mechanical logic that Ollman envisions?

These uncertainties do not imply that questions of capital accumulation and class power are to be ignored, as they are in so much of the political science literature on the state. Rather, they suggest the need to explain, rather than assume, the relation between larger logics of accumulation and the local mechanisms of state power.

It was in this context that my article emphasized the need to examine the micro, everyday practices that generate the effect of the state, drawing in part upon the work of Michel Foucault. Ollman and Bendix see this emphasis as an attempt to present an alternative to the macro analysis of the state; in fact, it was offered only as a place "to begin." The potential of Foucault's micro analysis of disciplines for developing a macro theory of the state is recognized by Nicos Poulantzas in his later writing, which is strongly influenced by works like *Discipline and Punish* (Foucault 1977). (American scholars tend to ignore the later work of Poulantzas; in fact, the English translation of *State, Power, Socialism* [Poulantzas 1978] has never been published in the United States.) But as Jessop (1990) points out in one of the best recent contributions to state theory, Poulantzas leaves unresolved the question of how the variety and incoherence one finds in disciplinary strategies at the micro level relates to the general pattern of power in the capitalist state. Foucault himself advocates what he calls an "ascending analysis of power," starting from its "infinitesimal mechanisms" and then examing how these come to be utilized, displaced, transformed, or maintained by "global mechanisms and the entire state system" (1980, 99–101). But he never actually conducts this examination, never explains how a global social order can be built out of such a diversity of infinitesimal strategies.

It was, in effect, to this gap in Foucault's work that the latter part of my paper was addressed. I proposed that the distinctiveness of those modern techniques of organization I termed "enframing" lay in the power to create an effect of order, abstraction, and totality out of mundane methods of distribution and control, an argument I have developed at greater length elsewhere (Mitchell 1988). Rather than deriving these forms of political power from the logic of capital accumulation, as Ollman proposes, it would be more appropriate to consider how capital itself is a phenomenon that is generated from these same methods of abstraction—methods of creating a seemingly abstract, nonpersonal frame of order, meaning, or value out of a diversity of infinitesimal processes of arrangement, representation, and control. One could then explore the relationship between capital and the modern state not by reducing one to the possessor of political power and the other to its instrument but by treating both as aspects of a more general discursive process of abstraction. These are questions that the kind of mainstream conceptualization of the state to which Bendix and Sparrow wish us to return would never enable us to pose.

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Notes

Sparrow thanks Mike Dennis, Laura Jackson, Bernard Silberman, and the members of the University of Chicago's Organizations and State-Building workshop and the University of Texas's American Politics workshop for their remarks.

1. This is not an argument for any particular variety of "American exceptionalism." For an overview of American exceptionalism, see Katznelson 1980, 3–17. See also the essays in Shafer 1991.

2. Miliband does not show why a state manager's individual actions need to be consistent with the collective dependence of the state on capitalist interests. Nor can Offe say why individual state actors should necessarily act in the collective interest of the state. Poulantzas, on his part, provides no microfoundations for the state's behavior, leaving the reader with a black-box state. Block, while providing for individual state actors, has difficulty in identifying "working-class pressures" and in dealing with the possibility that a state manager may default from acting in the general interest of capital. See Carnoy 1983 and Alford and Friedland 1984 for overviews of Marxian scholarship on the state.

3. Mitchell omits mention of Krasner's (1984) analysis of state-centered work. Krasner observes, "From the late 1950s until the mid-1970s the term state virtually disappeared from the professional academic lexicon" (p. 223). Krasner goes on to explore "the extent of state autonomy and the degree of congruity between the state and its environment" (p. 224). He proceeds to frame the statist approach in the context of the behavioralist revolution. Krasner thus not only touches on the boundary question but is also very much interested in the intellectual history of, and different perspectives on, the state.

4. Skowronek, on his part, defines the state to be at once composed by a coercive power settling class conflict at the national level; an "integrated organization of institutions, procedures and human talents" at the meso level; and individuals maintaining order at the micro level (1987, 11–12).

5. Nordlinger's (1981) *On the Autonomy of the Democratic State* is a thought experiment on state autonomy and intentionally omits empirical evidence.

6. Skocpol uses *the state* to refer to the societally autonomous cause of particular political outcomes—the state as an independent variable (Ikenberry and Skocpol 1987; Skocpol and Ikenberry 1983; Weir and Skocpol 1985). But *the state* may also refer to institutions mediating crisis or societal changes in order, again, to cause specific policies—the state as an intervening variable (Amenta and Skocpol 1988). Finally, the state may consist of the structures of government themselves, brought into being through preceding policies, particular alignments of political mobilization, and certain systems of representation—the state as a dependent variable (Skocpol 1979, 1981).

7. For Skowronek, "state building" consists of a convergence of tensions provoked by crisis, class conflict, and complexity—a convergence mediated by existing institutions and electoral politics.

8. See Burawoy 1989 for a criticism of Skocpol's (1979) methodology in *States and Social Revolutions*. Burawoy argues that Skocpol's inductivist method obscures the actual theory, that the method is unfalsifiable, and that it is ungeneralizable, that is, a summary of facts that subsequent, uncovered facts could disprove.

9. I say "point of departure" because research on organizations and institutions has gone in a number of directions: the transactions costs approach, with the economic transaction as the basic unit of analysis (e.g., Oliver Williamson, Douglass North); the study of law and economics, with a focus on the contract (Ronald Coase, Richard Posner); the new economics of organization, looking at principal–agent relations (Kenneth Shepsle, Barry Weingast, Terry Moe); and work in international relations (Robert Keohane, Stephen Krasner).

10. This "new," or "neo-,"institutionalism may be distinguished from the earlier institutionalism on the grounds of its response to the behavioralists' comparative neglect of social context and the durability of social institutions, an explicit concern with theory and methods, and a response to empirical anomalies—to wit, the lack of convergence between the study of organizations and politics and rational actor or functionalist accounts of these phenomena (Powell and DiMaggio 1991, 2–3).

11. For a more detailed exposition of this analysis see the chapters "Putting Dialectics to Work: The Process of Abstraction in Marx's Method" and "Theses on the Capitalist State" in my book, *Dialectical Investigations* (Ollman n.d.).

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FORTHCOMING IN MARCH, 1993

The following articles, research notes and controversies have been tentatively scheduled for publication in the March 1993 issue:

James Q. Wilson. "1992 Presidential Address: The Moral Sense."

Alberto Alesina, John Londregan, Howard Rosenthal. "A Model of the Political Economy of the United States."

David P. Baron. "Government Formation and Endogenous Parties."

John Dryzek and Jefferey Berejikian. "Reconstructive Democratic Theory."

Paul Huth and Bruce Russett. "General Deterrence Between Enduring Rivals: Testing Three Competing Models."

James Johnson. "Is Talk Really Cheap? Prompting Conversation Between Critical Theory and Rational Choice."

William Mishler and Reginald S. Songer. "The Supreme Court as a Countermajoritarian Institution? The Impact of Public Opinion on Supreme Court Decisions."

Roger B. Myerson and Robert J. Weber. "A Theory of Voting Equilibria."

Robert Powell. "Intrinsic Ends and Instrumental Means in International Politics."

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Robert H. Durr. "What Moves Policy Sentiment?" A Research Note.

Manus Midlarsky and Ted Hopf. "Polarity and International Stability." A Controversy.

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American Political Science Review Vol. 86, No. 4 December 1992

POLITICAL REGIME CHANGE: STRUCTURE AND PROCESS-DRIVEN EXPLANATIONS?

HERBERT KITSCHELT *Duke University*

The Faces of Fraternalism: Nazi Germany, Fascist Italy, and Imperial Japan. By Paul Brooker. New York: Oxford University Press, 1991. 397p. $69.00.
To Craft Democracies: An Essay on Democratic Transitions. By Guiseppe Di Palma. Berkeley: University of California Press, 1990. 248p. $35.00 cloth. $11.95 paper.
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Liberalism, Fascism, or Social Democracy: Social Classes and the Political Origins of Regimes in Interwar Europe. By Gregory M. Luebbert. New York: Oxford University Press, 1991. 416p. $49.95 cloth, $19.95 paper.
Capitalist Development and Democracy. By Dietrich Rueschemeyer, Evelyne Huber Stephens, and John D. Stephens. Chicago: University of Chicago Press, 1992. 387p. $45.00 cloth, $19.95 paper.

Political regimes may be defined as the rules and basic political resource allocations according to which actors exercise authority by imposing and enforcing collective decisions on a bounded constituency. Over the past decades, profound political regime transformations in southern Europe, Latin American, East Asia, Eastern Europe, and perhaps now even Africa have revived political scientists' concern with the breakdown and replacement of political governance structures. The main theoretical division within this field is drawn between those who seek more "structural" and "configurational" explanations, on the one side, and those who focus on the process of change itself—the sequence of events and the strategic moves of the actors.

The differences between the two camps encompass theory, methodology, and research design. Structure-oriented scholars typically assume that historical actors face extremely narrow choice sets *or* that their rational choices are clearly constrained by the distribution of resource endowments and their exogenous interest to maximize income and/or power, in light of which they calculate optimal strategies. Process-oriented scholars, in contrast, are primarily interested in the actors' manipulation of their own and their adversaries' cognitive and normative frames. Such change may eventually bring about political regime changes that were neither anticipated nor desired by any of the participants at the beginning of the process.

In other words, the main division in the field of studies on regime transformation is *not* one between historical and political institutionalists, on the one hand, and economic institutionalists with rational choice models, on the other. These two currents represent only strands within the structuralist camp. At most, the two differ on the nature of actors' interests (self- or other-regarding) and the relationship between interests and structural constraints (exogenous or endogenous). What is at stake between structure- and process-oriented studies of political regime change is a more fundamental division concerning the *concept of choice in political action itself*. For structuralists, choices represent calculations in light of given preferences and institutional constraints.[1] For process-oriented scholars, choices are caught up in a continuous redefinition of actors' perceptions of preferences and constraints.[2]

I shall focus the debate within the structuralist camp and between structuralists and process-oriented approaches on the question whether Barrington Moore's seminal hypotheses about regime trajectories in *The Social Origins of Dictatorship and Democracy* (1966) can still be considered valid. In a nutshell, Moore argues that the deconcentration of political power and economic power before the advent of industrialization (precipitated by a commercialization of land, a rising bourgeoisie, and a fragmentation of state power) was beneficial to democratization. At least four of the five structuralist and process-oriented approaches I shall examine address these hypotheses directly or indirectly.

Structure- and process-oriented approaches also differ on methodological and design-related questions. Structuralists prefer systematic macroquantitative or conceptually disciplined qualitative comparison, with countries as units of analysis. As the books by Huntington, Luebbert, and Rueschemeyer, Stephens and Stephens testify, followers of the comparative case study methodology have also ventured to increase the number of cases they compare and thus to lend more credibility to their main theoretical contentions.[3] In contrast to structural approaches, the process-oriented literature revolves around descriptive diachronic reconstructions of individual cases of regime transition with very little systematic comparison across a wider universe of countries. Whatever comparison is brought in remains on the illustrative and impressionistic level. I know of no single process-oriented study that supplies a systematic comparative analysis of regime transitions for a large number of instances.[4]

I wish to argue that structuralist and process-oriented analyses do not directly compete with each other but focus on different objects of explanation with different research methods and comparative designs. Taken as absolutes, structuralist approaches tend to explain "too much," whereas process approaches explain "too little." Structuralist approaches are good at accounting for the general causes of regime breakdown and the consolidation of new regimes. Process approaches may explain the timing of breakdown and transition as well as the specific trial-and-error process of searching for a new viable regime. Structuralists have little to say about timing and transitions; nor have process-oriented studies succeeded in predicting regime consolidation. As far as the probability of democratic consolidations is concerned, a modified version of Moore's arguments still appears to have considerable plausibility.

STRUCTURALIST APPROACHES

Gregory Luebbert concludes his study of regime change in the interwar period with an unabashed structuralist proclamation: "One of the cardinal lessons of the story I have told is that leadership and meaningful choice played no role in the outcomes" (p. 306). Drawing upon a wide range of cases essentially covering the entirety of Europe, including the Balkan states, Luebbert claims that it is class cleavages and class alliances articulated on the level of group and party mobilization that determined whether regimes became fascist, liberal democratic, or social democratic in the interwar period. In a nutshell, the strength of liberal democratic free-market-oriented parties before and after World War I is key. Such parties were furthered before World War I by the absence of religious, ethnic, or regional cross-class cleavages that could have divided nonsocialist voters. Dominant liberal parties were able to enter alliances with the emerging labor parties against entrenched conservative and antidemocratic elites and thus weakened and coopted the labor movement in a democratized capitalist order (Britain, France, Switzerland). Whereas strong liberalism with weak labor movements persevered during the interwar decades of economic crisis, divided bourgeois parties and strong labor mobilization before and after World War I led to fascist regimes where the peasantry threw in its lot with the reaction (esp. Germany, Italy, Austria, and, to a lesser extent, Spain) and to social democracy where the peasantry entered red–green coalitions (Scandinavia). In eastern and southern Europe, mass political mobilization, particularly that of the working class, had not proceeded far enough to yield anything beyond an authoritarian dictatorship, regardless of whether fascism, liberalism, or social democracy are considered (pp. 258–66).

Whereas Barrington Moore saw large landholders and labor-repressive agriculture as a key force promoting fascism in the twentieth century, Luebbert maintains that by the 1920s, the German Junkers no longer played a critical economic role and that the landed elite did not control the votes of rural laborers anyway (p. 309). Luebbert's argument, however, fails to recognize that an application of Moore's thesis to twentieth-century regimes must necessarily emphasize the imprints past class dominance had left on the political organization of the state and on the mobilization of the peasantry. In fact, where feudal and absolutist regimes based on large landowners had disappeared early, fascism had no basis for success because either the state organization (military, bureaucracy, judiciary) was inimical to a new authoritarianism or a class of family farmers had an independent political voice that usually favored democratization, or both.

The critical cases undermining Luebbert's arguments are Belgium and the Netherlands, included among the countries he classifies as without hegemonic liberalism before World War I; yet contrary to his theoretical expectations, they did not produce fascism in the interwar period. Both countries are, however, characterized by the early demise of feudalism, the dominance of family farms, and a large market-based middle class.

Luebbert's argument that late industrialization has nothing to do with the failure of liberalism (pp. 59–62) also rests on weak foundations. Timing may not be as important as the character of late industrialization in critical countries in order to account for regime outcomes. Late industrialization is often associated with an initially state-led process of industrialization yielding a highly "organized" capitalism with a bourgeoisie closely intertwined with an antidemocratic state apparatus. If there were no offsetting conditions, such as an independent liberal–democratic peasantry (Sweden), this pattern of industrialization favored authoritarianism (Germany, Japan). Again, this interpretation explains why Belgium and the Netherlands escaped authoritarianism in spite of cross cleavages dividing liberal parties. Both countries experienced a gradual and relatively spontaneous, decentralized process of industrialization.

Although Luebbert is a structuralist, he is forced to admit the contingency of political choice in at least one critical instance. He argues that whether or not farmers allied with the Left or with fascism essentially depended on the socialists' choice to mobilize farm workers. If socialists engaged in that strategy, they tended to unite midsized farmers and large landholders against the Left and thus prepared the way for a fascist coalition, whereas in other instances red–green alliances were feasible (pp. 277–95).[5] In the spirit of Luebbert's structuralism, however, it must be noted that countries where peasants coalesced with fascism had a different legacy of agricultural property rights than countries where they did not. Conversely, if historical patterns of landholding and their political consequences did not matter, political choice on the part of socialist party leaders is clearly critical in accounting for interwar regime outcomes. Luebbert cannot have it both ways, that is, both deny the political consequences of rural political economy and provide a structuralist account of interwar regime outcomes.

Overall, Luebbert's account convincingly demonstrates that structures matter, but he may push this point too far. For example, the *timing* of the antidemocratic relapse in Germany, Italy, or Spain could hardly be accounted for by his model. Moreover, structuralism yields no deterministic, but a probabilistic, prediction. Even if all the structural constraints were right, would there have been a rise of the German Nazi party to power in 1933 had it not been for the Depression and the unique form of elite politics ushered in by the provisions of the Weimar constitution?

Luebbert's historical account yields rather pessimistic conclusions for the future of democratization in the late twentieth century. Liberalism and democracy were compatible only before the working class acquired the capacity to mobilize and disrupt the operation of free markets early in this century. Liberal–Labor alliances, modeled on pre–World War I Britain or France, between the urban middle class and the working class are thus extremely unlikely (p. 315). At the same time, a rural middle class was critical in the past and does not appear to be present in most countries still facing the possibility of democratization. Luebbert also does not hold out much hope for a "tutelary," restrained democratization under elite control because it will only compound the problems of democratic stabilization (p. 313). Under these circumstances, he envisions instability as the only "stable" outcome, "a cycle of establishment of democracy and its collapse, military intervention, and withdrawal" (p. 315).

Luebbert's book is an impressive achievement in its comparative scope and its unrelenting, tenacious defense of a clear analytical thesis; yet it ultimately leaves too many empirical anomalies unaddressed, maybe because Luebbert did not take Barrington Moore seriously enough. In contrast, Rueschemeyer, Stephens and Stephens's *Capitalist Development and Democracy*, though analytically more complex and less tightly knit, incorporates more aspects of Moore's thinking and arrives at more plausible and valid explanations of an even wider range of cases than Luebbert considered. The authors' objective is to synthesize two hitherto antagonistic traditions in research on regime change and apply the synthesis to regime change in today's advanced industrial countries (Western Europe and British settler democracies), Latin America, and the Caribbean. Macroquantitative studies have consistently found that economic development furthers democracy. Comparative historical analyses in the political economy tradition from Weber to Moore, however, have emphasized class relations and political institutions. Rueschemeyer and his coauthors argue that comparative historical political economy provides the *mechanisms* that explain why, when, and how economic development translates into democracy. Moreover, political economy can explain the cases that remain "outliers" in macroquantiative analysis.

Greater explanatory specificity in accounts of regime transformation requires examination of three sets of variables. First, class interests and class alliances matter, but in ways that modify Barrington Moore's master thesis. Consistent with Moore, Rueschemeyer, Stephens and Stephens show that labor-intensive and labor-repressive agriculture under the direction of large landowners is inimical to democracy. They also demonstrate that in the late nineteenth and early twentieth century the political effects of long-term political hegemony of landowners on state structures (military, bureaucracy, judiciary) may be as important for political regime forms as the direct impact of agricultural property rights.

Contrary to Moore's thesis, Rueschemeyer, Stephens and Stephens argue that the bourgeoisie is generally inimical to democracy. The true source of persistent democratic drives is working-class mobilization, which, in combination with middle-class support, can bring about a political constellation favorable to democracy. The authors qualify their emphasis on the working class in several respects. When turning to Latin America, they note that the middle class is critical in bringing about full democratization (pp. 167, 181, 216–7). Even for advanced industrial societies, however, the subordinate role of the middle classes may be questioned. If the working class is mostly for democracy, it is really the middle class—a concept the authors define as "urban professionals, state employees and employees in the private sector, artisans and craftsmen, and small entrepreneurs, sometimes joined by small and medium farmers" (p. 185)—whose variable position determines regime outcomes. A good part of Rueschemeyer, Stephens and Stephens's middle class may be covered by Moore's concept of bourgeoisie, thus reducing the gap between the two accounts. Going beyond simple structural accounts of stratification, Rueschemeyer, Stephens and Stephens emphasize that class formation is an organizational process, rather than a political–economic configuration of property rights. But they might have further developed this insight in systematic comparisons of working-class, "middle"-class, and bourgeois-class formation to highlight their position vis-à-vis Moore.

The other two sets of variables explicitly theorized in Rueschemeyer, Stephens and Stephens's explanatory framework are state structures and transnational power structures. In general, the more resources state elites control independently of socioeconomic classes and the more they represent a hierarchically integrated and ideologically united state apparatus, the more likely it is that authoritarian regimes will take hold. The precise nature of state influence depends on the countervailing force of independent voluntary associations ("civil society") and the nature of the dominant class from which the state is autonomous. Also, transnational power structures have highly contingent effects. In general, dependency in the periphery of the world system involves mechanisms unfavorable to democracy (foreign direct investment, capital-intensive and sectorally constrained industrialization, or labor-repressive and labor-intensive agriculture). But the authors also show that British colonialism and its legacies in state structures also had indirect effects that kept the "British" Caribbean, with few exceptions, democratic in the post–World War II era, whereas the "hispanic" Caribbean under U.S. influence experienced some of the most repressive and exploitative dictatorships of that era. In addition, other international factors, such as war, the Cold War, and alliance systems, have significant but varied effects on regime change.

Whereas these three sets of variables (particularly class structures and alliances) tend to account for democratization in advanced industrial countries very well and also cover the four cases of authoritarian breakdown (Italy, Germany, Austria, and Spain [pp. 99–121]), the discussion of Latin America and of the Caribbean requires a further set of variables, namely, the institutionalization of political parties. The authors discover that only where parties are institutionalized as lasting competitive organizational alternatives (whether based on clientelistic linkages or programmatic differences) do democracies survive for any length of time. It is somewhat unfortunate that the systemic role played by parties is not emphasized also for Western democratization. Characteristically, in Germany, Italy, and Spain the nonsocialist parties did not solidify around firm party organizations in the interwar period and were easily swept away by fascism. Austria is the exception, but here a full-fledged fascism was imposed only through Germany's annexation of the country. The importance of parties in twentieth-century political regime change would have allowed the authors to marry Moore's and Luebbert's central insights in a more encompassing framework.

The authors touch upon the role of ideology in regime change briefly but postulate that it is "linked to structural and organizational realities" (p. 50). In particular, this applies to religion. While the authors maintain that religious organization is more important than church doctrines (p. 275), the two elements are usually so highly intertwined that it is not clear in which direction the causal arrow runs. Moreover, lacking a close analysis of countries beyond the reach of Christian doctrines and church organizations, it is hard to determine how important either aspect of religion is for political regimes.[6]

Given the complexity of the variables Rueschemeyer, Stephens and Stephens take into account, what is the analytical bite of their study? What events and trajectories of regime change does it rule out? The discussion of the Latin American cases (preceded by a 12-page summary of the main argument), in particular, is so complex that it is not entirely clear in the end what has been achieved in terms of explanatory parsimony. Rueschemeyer, Stephens and Stephens do wish to argue against a facile modernization theory associating industrialization with democracy; yet at the same time, they embrace it as a partial explanation. They emphasize the role of the working class but do not get around the role of the middle class. The discussion of state structures and transnational relations leaves so many options and contingencies open that just about anything appears to be possible. One wishes the authors would have explored some less complex structural explanations, such as Ronald Rogowski's elegant and provocative account of political cleavages and regime alternatives in *Commerce and Coalitions* (1989) to show that a complex approach still combines the virtues of greater empirical verisimilitude and of discrimination between predicted, possible, and impossible pathways of regime transition.

At times, one has the impression that Rueschemeyer, Stephens and Stephens try to convert every contingency into a structural historical determinacy in order to exorcize the role of a process-oriented account of regime change. Particularly in Latin America, where it is often hard to say whether regimes ever "consolidated," this standard may be driving the structuralist approach too far. Rueschemeyer, Stephens and Stephens argue against the "presentism" of process-oriented explanations of democratic regime transitions (p. 35); but they do not confront these explanations with their own findings. The complexity of their account is rooted in the ambition to explain regime trajectories exhaustively and represent complex configurations and sequences of causation. In light of the result documented in their book, however, more than a few readers may conclude that the isolation of individual mechanisms affecting political regimes and the selective consideration of contingencies to account for outliers promises to provide a more manageable structuralist research strategy than explanatory holism. Nevertheless, Rueschemeyer, Stephens and Stephens have produced a splendid book that will definitely further theorizing on political regime change.

In the spirit of Moore, there is one very important hard-and-fast message that consistently comes across in Rueschemeyer, Stephens and Stephens's study: countries with labor-intensive and labor-repressive agricultures will not become stable democracies. This message also influences their concluding speculations about the future of democracy. In Taiwan and South Korea, the absence of a landlord class and rapid industrialization combined with the strength of autonomous states yields moderately favorable prospects for democracy. The decline of agriculture has also brightened hopes in Latin America; but the weakness of working-class organization, state and military autonomy, dependence on foreign capital, and the lack of strong parties cast dark shadows over the future of democracy on that continent. In Eastern Europe, finally, the *nomenklatura* may constitute the functional equivalent of a landlord class and the large autonomous state apparatuses within weak civil societies also do not bode well for democracy. Even in the early 1990s, structuralists like Luebbert or Rueschemeyer, Stephens and Stephens arrive at muted-to-pessimistic conclusions about the future spread of democratization.

PROCESS-ORIENTED APPROACHES

Whereas structuralists hold actors' preferences constant and focus on changing institutional constraints, process-oriented studies of regime transformation emphasize the contingency of all parameters of choice. In this regard, Paul Brooker's *The Faces of*

Fraternalism represents an analysis at the crossroads between the two perspectives. It is emphatically rooted in Durkheimian sociology in its emphasis on the changing role of a comprehensive collective conscience/consciousness for societal integration and regime stability. Yet Brooker is careful to explain in the first three chapters of his study that efforts to revive a communitarian spirit by "fraternal" political regimes in Germany, Italy, and Japan in the first half of the twentieth century cannot be explained in social-structural terms. Fraternal appeals did not initially inspire a mass following; nor did it necessarily come to the fore before, or even soon after, national socialism, fascism, and its Japanese military variant came to power. Thus, contrary to Durkheim, fraternalism does not grow out of a segmental division of labor with mechanical solidarity but somehow emerges in advanced, functionally differentiated societies with organic solidarity, something that would appear to be an anachronism to Durkheim. For this reason, Brooker offers a quasi-rational explanation of fraternalism in twentieth-century politics. In order to consolidate domestic regime entrenchment and to mobilize psychological resources for their external imperialist plans, totalitarian regimes decided to engage in fraternalist appeals and mass organizations (p. 74). Over more than two hundred pages, Brooker then provides a detailed account of the organizations and mechanisms each of the three regimes employed to instill fraternalism in the population. The concluding assessment argues that fraternalism was carried furthest in Japan yet remained more or less unsuccessful in Germany and Italy. In the most successful case, however, structural conditions for fraternalism were most conducive.

Brooker warns his readers on the first page of the preface that "most political scientists and sociologists will view the book as conceptually lightweight" (v); and this, indeed, is the book's major problem. Its conceptual apparatus and explanatory thrust could have been strengthened by engaging in a variety of related or competing theoretical approaches to fascism and to the rise of segmental or "pillarized" mass parties and reactionary social movements in order to explore why these, but few other, countries were vulnerable to the lures of a totalitarian order appealing to a new spirit of particularist (racist, nationalist) community. In terms of comparative design, Brooker samples on the dependent variable. Fraternalist regimes would appear in a much starker light if they were contrasted to regimes that failed to become fraternalist when faced with similar decision problems and political crises. Why is it that fraternalist appeals struck a resonant cord within elites primarily in Germany, Italy, and Japan but not in most other industrial countries? Brooker does not answer this question. In the end, Brooker provides an instructive description of the internal institutions of totalitarian rule in the three countries but little more. The book's greatest weakness, in fact, may be that it does not discuss alternative structuralist comparative accounts of regime change, such as the strand of analysis to which Barrington Moore had contributed.

Guiseppe Di Palma's *To Craft Democracies* has rather different ambitions and is firmly rooted in the "proceduralist" view of democratic regime change. Not structural conditions but human actions achieve democratic consolidation. At the center of Di Palma's concerns is the process of "democratic crafting" involving "negotiated agreements" between challengers of the old order and incumbent elites that move common perceptions of self-interest toward accepting democracy as the best possible regime form under given conditions. The establishment of democracy, unlike that of other systems, does not legislate a particular distribution of resources among social groups, but institutes uncertainties over outcomes as Adam Przeworski (1991) argued in *Democracy and the Market*. For this reason, Di Palma believes that the game between authoritarian elites and challengers can be more easily converted into a positive sum game, provided that the right steps in the process of regime transition are undertaken. Authoritarian elites must be assured of "coexistence" and fair play. For this reason, the transition should be engineered through negotiated agreements (pacts) that focus on rules of decision making, rather than outcomes. Demonstration effects and the diffusion of successful examples from abroad may be important in precipitating such steps. Careful crafting can exploit the elites' internal divisions on strategy and their sense of uncertainty about their own interests. Di Palma demonstrates these strategies of *garantismo* with an analytical reconstruction of the Italian and Spanish experience and contrasts it to the Portuguese case, where democratization almost failed.

Di Palma then discusses a number of tactical devices that facilitate the transition process. In addition to pacts, a speedy timetable of electoral reform and balanced concessions among all participating forces pave the road to democracy. Over time, the rules that are instituted generate their own support, once actors devise their strategies around the new opportunity structures. For this reason, "legitimacy," as a normative belief in the correctness of the system of rules, is certainly not a precondition of democracy. Institutional habituation of actors' strategies to democratic procedures is the overriding goal of democratic crafting.

While not denying that tactical moves on the part of democratic challengers and skilled techniques of converting games into positive-sum payoff structures facilitate a transition to democracy, we must ask how far such processes carry us in explaining democratic consolidation? Much of his attack on structuralism targets a vague modernization-theoretic version that stresses democratic values and elite attitudes yet not the more hard-nosed comparative studies in the tradition of Moore and others that focus on property rights, class relations, and state institutions. After Di Palma raises some rather strong claims for procedural analysis in the opening chapters of his book, he takes much of them back in chapter 8 where he begins to compare, in a rather impressionistic and unsystematic manner, the structural conduciveness of different regions and countries to democratic crafting. Such crafting, taken by itself, is insufficient to explain democratic prospects in particular cases: "Even when we favored explanations that focus on the transition and its strategies, these in turn have begged for their own explanations—often of a deeper historical and structural nature" (p. 156). The final chapter of the book also raises the importance of international system configurations and concedes that hegemonic powers play a critical role in regime change.

In the end, Di Palma's book provides several interesting insights into transition techniques that may make a difference in countries characterized by roughly similar structural conduciveness to democratization. These observations, however, could be analytically sharpened. For examples, Di Palma's study puts a great deal of emphasis on pacts and agreements between incumbent and challenging elites yet never engages in a specific analysis of the different scope, bindingness, timing, and longevity of pacts in the transition process. More importantly, the book does not deliver a specification of the structural circumstances that may give significance to different techniques of crafting for the ultimate outcome. Di Palma delivers a timely reminder that economic, political, and cultural determinants are not everything when we try to understand regime change but does not present a sufficiently crisp and precise analysis, corroborated through systematic comparison, to advance the emerging field of "transitology" in significant respects. Whereas structural approaches explain too much, Di Palma's "transitology" explains too little. For this reason, Di Palma's optimism requires a more robust theoretical base.

COMBINING STRUCTURAL AND PROCEDURAL ACCOUNTS OF DEMOCRATIZATION

Samuel Huntington, in *The Third Wave*, lays out a broad, eclectic canvass that encompasses structural and procedural arguments. He also considers the most comprehensive universe of cases and treats them in a more rigorous comparative analysis than does Di Palma; but he is less systematic than Luebbert or Rueschemeyer, Stephens and Stephens in tracing the impact of different sets of variables on regime patterns. Huntington resists efforts to pigeonhole his account as "nomothetic" or "ideographic" from the very beginning (P. xiii). Democracies are complex phenomena, and they are caused by many forces' contributing to historical "waves" of democratization. In contrast to the first two waves following on the heels of the two world wars in the twentieth century, the third wave of democratization beginning with the 1974 Portuguese revolution appears not to be triggered by international political events but by a variety of domestic forces. It is important to note that that 23 of the 39 countries democratizing since 1974 had at least one previous democratic experience, whereas most countries that had never been democratic stayed authoritarian until 1990 (p. 44). This association reestablishes a linkage to externally induced democratization in previous waves.

Overall, Huntington sees five causes of the third wave—at least three of them structural but one unambiguously procedural. First, authoritarian regimes are unable to dissociate the regime's economic effectiveness from the legitimacy of rulers or the legitimacy of rulers from legitimacy of rules of governance. Hence, the oil shocks and the economic dislocations and structural transformation of the world economy in the 1970s and 1980s precipitated the downfall of many dictatorships. With the decline of communism, "outside of Africa and a few countries elsewhere, democracy had come to be seen as the only legitimate and viable alternative to authoritarian regimes of any type" (p. 58). Second, Huntington stresses the importance of economic development but sees it as neither a necessary nor a sufficient condition. Going beyond older modernization approaches, he argues that the source and distribution of wealth are important. Hence, the oil-rich, state-centered regimes did not become democratic; but countries that generate a broad-based urban middle class through a wide diversification of a growing industrial economy did. Huntington's analysis is here quite close to Rueschemeyer, Stephens and Stephens's main hypothesis and certainly consistent with the overall gist of Moore's earlier analysis. Third, however, Huntington emphasizes the independent effect of religious doctrine and religious change. Reversing Weber's causal chain, he notes the rise of Christianity—particularly an individualist and socially activist Christianity—in democratizing countries that has forced Catholicism to withdraw support from the political status quo. Fourth, beyond these structural changes, new transnational politics effected by the United States, the Soviet Union, the European Community, and the Vatican improved the atmosphere for democratic change in the 1980s. Finally, the democratic wave began to feed on itself. Demonstration effects and snowballing reinforced opposition efforts to bring about democracy in countries where structural conditions were even mildly conducive to regime change.

Huntington shows that the mode of transition interacts with the nature of incumbent political regimes (p. 133); thus, he goes significantly beyond the structuralism/proceduralism divide. Elite-initiated or elite-negotiated transitions are most typical in bureaucratic one-party or military regimes, whereas personal "sultanistic" dictatorships hold out less promise for change and often require the wholesale replacement of the ruling elite. For each of the patterns of transition, Huntington provides detailed descriptions of actors' moves and tops off each analysis with a set of lessons actors might draw. For example, reformers should always participate in elections, because even rigged elections, monitored by foreign observers, weaken an authoritarian regime. In any case, rulers usually miscalculate their chances in open elections, except in cases where there is only a small urban middle class (p. 179).

Huntington finally discusses various aspects of democratic stabilization, such as how to decide on the prosecution of authoritarian elites and how to deal with the military. The most interesting segment deals with the prospects of democratization of countries that have made initial steps in the third wave of democratization (pp. 270–79). Based on their previous democratic experiences, economic development, location in the international system, and duration of democratization effort in the third wave, Huntington is least optimistic about Mongolia, Sudan, Pakistan, Nicaragua, Romania, Bulgaria, Nigeria, and El Salvador.[7] He remains especially pessimistic about democratic prospects in regions of the world that have not entered democratization, especially homegrown Marxist–Leninist regimes linked to nationalist appeals, sub-Saharan Africa, Islamic countries, and certain areas of East Asia. Huntington makes a special point to highlight the antidemocratic implications of Confucian and Islamic religious doctrines, a factor not considered by any of the other studies.[8]

Huntington's analysis is impressive in scope; but at times he is compelled to trade sophistication in conceptualizing critical variables for breadth of comparison. Rueschemeyer, Stephens, and Stephens's account of economic development, class structure, and democratization tends to go in the opposite direction. Clearly, Huntington's book does not provide as detailed an analysis of political institutions and regime structures preceding democratization (pp. 110–21). Huntington's global reach, however, has its own virtues; it encompasses structure and process and, due to its global reach, is able to say more on such contentious issues as the role of religion for political regime transformation.

Overall, the two studies from which most can be learned for the further analysis of regime change are Huntington's and Rueschemeyer, Stephens, and Stephens's. Luebbert's study is a close runner-up. All of these analyses in one way or another feature the importance of Barrington Moore's insights and of the tradition on which Moore built. All of them also refrain from extreme optimism or pessimism about the future avenue of global democratization. Huntington and Rueschemeyer, Stephens, and Stephens lead us to hypothesize that in the Middle East or sub-Saharan Africa, but also in some of the postcommunist successor regimes, chances for democratic consolidation are poor. Conversely, there are a number of countries and regions of the world that give reason for hope to those who welcome democracy.

Against the backdrop of these studies, what future avenues may be open for analyzing political regime change? Luebbert and Rueschemeyer, Stephens, and Stephens have probably pushed the case-oriented systematic historical–comparative approach as far as it can productively go. Conversely, Huntington offers a broad, variable-oriented comparison, which still remains bivariate in its consideration of causal elements. In the future, multivariate analysis or Boolean algebra, as proposed by Charles Ragin (*The Comparative Method* 1987) may help us to develop a more formal representation of casual complexity in pathways of regime transition. At the same time, it is desirable to spell out more clearly the precise mechanisms that make each causal force relevant if we are to see sharper analysis of regime transitions.

Notes

1. For examples, Ronald Rogowski's *Commerce and Coalitions* (1989) is an avowedly rational-choice-based account; yet it considers itself to provide a generalization of Gerschenkron's and Moore's comparative-historical class-based structuralism. Conversely, Theda Skocpol's *States and Revolutions* (1979) is explicitly based on structuralist premises yet implicitly assumes rational actors who maximize wealth or power and are not driven by more illusive ideas.

2. Sometimes contributors to studies of regime transformation misunderstand their own accomplishments. In this sense, Adam Przeworski in *Democracy and the Market* (1991) wishes to offer a rational-choice-based explanation of regime change yet really falls back onto a historical institutionalism *plus* a process-oriented reconstruction of actors' changing cognitive frames.

3. Rogowski's book (see n. 1) is the other major examples of this more comprehensive comparative approach.

4. Probably closest to the systematic and encompassing comparison of transition processes I am looking for are Terry Karl's articles, especially "Dilemmas of Democratization in Latin America" (*Comparative Politics* [1990]) and (with Philippe Schmitter) "Modes of Transition in Latin America, Southern and Eastern Europe" (*International Social Science Journal*, May 1991). In these and other examples, however, the conceptualization of the main independent variable, modes of regime transition, is not sufficiently refined.

5. I am setting aside here the question whether it is still adequate in light of recent historical research to refer to the rise of Scandinavian social democracy as a "red–green" coalition of workers and farmers against the bourgeoisie. As Swenson's research makes clear, there emerged, if anything, a sectoral coalition in which parts of Labor and business cooperated with agriculture against Labor and business in other sectors. See Peter Swenson's "Bringing Capital Back In" (*World Politics* 43[1991]).

6. To explore the influence of religion, see especially the broad comparative neo-Weberian account in John Hall's *Powers and Liberties* (1985).

7. I have added up rankings of the countries on each of Huntington's four criteria and singled out countries with the four lowest rankings. At the top of democratic promise are Spain, Greece, Portugal, Turkey, Uruguay, Chile, Ecuador, Czechoslovakia, Hungary, Bolivia, Brazil, Peru, Philippines, and South Korea.

8. According to Huntington, even formal democracies in predominantly Confucian countries are characterized by the absence of government turnover (p. 304).

POLITICAL THEORY

Nietzsche Contra Rousseau: A Study of Nietzsche's Moral and Political Thought. By Keith Ansell-Pearson. New York: Cambridge University Press, 1991. 284p. $49.95.

A considerable number of specialized scholarly studies of Nietzsche's political thought have appeared in the last few years, a growth of interest in the subject that clearly reflects the recent revival of Nietzsche's reputation among contemporary scholars. *Nietzsche Contra Rousseau* stands out among these studies for the clarity and soundness of its interpretations, as well as for its greater familiarity with the best of German language scholarship on Nietzsche.

Ansell-Pearson uses Nietzsche's critique of Rousseau as a way of identifying and exploring the central issues in Nietzsche's moral and political thought. This dialogic approach to interpretation has many advantages, especially since Ansell-Pearson gives Rousseau a chance to respond to Nietzsche's criticisms and thereby help us identify Nietzsche's own limitations. It is particularly helpful in identifying the most important features of Nietzsche's understanding of the relationship between resentment and democratic politics. As Ansell-Pearson shows, Rousseau's obsessive and self-dramatizing moral indignation represents for Nietzsche the hidden core of democratic moralism: resentment against the fundamental conditions of human existence.

Ansell-Pearson develops this point in his excellent chapter on Nietzsche's *The Genealogy of Morals*. He presents in this chapter an especially interesting and original account of Nietzsche's understanding of justice and community. This account effectively demolishes the widespread image of Nietzsche as an ethical and political individualist.

Bringing Nietzsche into dialogue with Rousseau also helps Ansell-Pearson identify the sources of Nietzsche's own resentment against the world. Nietzsche, of course, preached the overcoming of resentment and the birth of a "second conscious innocence" toward the cruelties of human existence. But as Ansell-Pearson effectively shows, Nietzsche's own bitterness toward modern individuals and society led him to delude himself in many ways. In particular, Nietzsche's hatred of the modern world led him to believe that modern degeneration and corruption might itself provide the basis for the greatest and most life-enhancing transformation of human culture. Rousseau, by contrast, was far more realistic. He recognized that if society had completely corrupted human character, then there was little to hope for from revolutionary upheavals but grand displays of vice and cruelty.

This Rousseauian response to Nietzsche can be pushed even further. Rousseau had the great advantage over Nietzsche in being quite self-conscious about his own resentments. The contrast between Rousseau's *Reveries* and Nietzsche's *Ecce Homo* as autobiographical testaments is quite instructive and none too flattering to Nietzsche.

Ansell-Pearson's dialogic approach to interpretation does not, however, serve him as well when he begins to assess the value of the arguments explored in the book. Building on the Rousseauian critique of Nietzsche just mentioned, he points us toward a synthesis of Rousseauian and Nietzschean politics, "a Dionysian politics that is both agonistic and democratic" (p. 18). But in order to consider this vision of politics seriously one would have to step further outside of the Nietzsche–Rousseau dialogue than Ansell-Pearson seems willing to do.

Other political thinkers have explored the possibility of a "politics that is both agonistic and democratic." Hannah Arendt, in particular, springs to mind. Her efforts, as well as the internal tensions within such a view of political life, suggest a number of critical questions about the possibility of realizing such a goal.

How, for example, can one maintain an "agonistic" view of politics without developing aristocratic contempt for the mass of the people who are unable or unwilling to participate in political struggles for recognition? Many would argue that Arendt's attempt to synthesize Rousseau and Nietzsche fails precisely because she ends up expressing such contempt. Alternatively, how can one value all members of a democratic community without developing moralistic resentment toward the most creative and powerful individuals, the kind of resentment that Nietzsche argues poisons the minds of Rousseau and all other democrats? Is there a nonmoralistic way of defending the view that every individual deserves a certain basic level of respect and dignity? *Nietzsche Contra Rousseau* would have been a much more interesting book had Ansell-Pearson stepped back a little from his dialogue between Nietzsche and Rousseau to consider these important questions more fully.

Nevertheless, Ansell-Pearson's reconstruction of that dialogue makes for interesting reading. It provides a very stimulating and useful way of identifying some of the most important questions raised by Nietzsche's political thought.

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Justice, Law, and Violence. Edited by James B. Brady and Newton Garver. Philadelphia: Temple University Press, 1991. 368p. $44.95.

Violence, Terrorism, and Justice. Edited by R. G. Frey and Christopher W. Morris. New York: Cambridge University Press, 1991. 319p. $54.50 cloth, $17.95 paper.

Almost everyone condemns violence, but there agreement ends. There is a broad consensus among writers on the subject that unlike "force," which may be used for good or ill, "violence" is necessarily objectionable, at least prima facie. Violence has a case to answer. Beyond that narrow strip of common ground, disagreements multiply. Whether violence can ever be justified—and if, so how; whether some forms of violence are worse than others; whether violence is, on the whole, diminished or exacerbated by governments and legal systems; and even what, exactly, "violence" is—these are questions that excite vigorous and sometimes partisan dispute.

This diversity of opinion is well (although no doubt not comprehensively) reflected by these two volumes.

Of the two, Brady and Garver's *Justice, Law, and Violence* casts the wider net, grouping its contributors under three major headings. "Violence" deals with the nature of the beast and attempts to define it; "Law and Legitimation" examines putative justifications of violence, especially as used in the service of law; and "Controversial Instances of Allegedly Legitimate Violence" raises issues concerning punishment, terrorism, and domestic violence. Individual contributions are consistently thoughtful and interesting, although they vary in length and clarity. The presence of writers in the Continental tradition, as well as the Anglo–American, is, predictably, a mixed blessing, providing the usual combination of challenging extremes and murky prose.

Two such writers give some idea of the range of views to be found here. Sergio Cotta argues that violence is "the absence of measure," whose content is given by natural law. Although objective moral standards may sanction the use of force, "violence" consists in force that goes beyond what is permitted by such standards. The perceived increase in violence in the contemporary world (Is there really such an increase?) is due, most importantly, to the abandonment of objective norms in favor of a cult of subjectivity that takes the individual self as the sole author and judge of value. For Bernard Waldenfels, on the other hand, violence is *not* fundamentally opposed to legal and moral norms. On the contrary, violence is inextricably intertwined with law and, indeed, with justification itself. In exactly what sense this is so is not entirely clear. Does he mean that since justifications are always incomplete, remaining open to objection, laws will always require enforcement? But Waldenfels seems to want to go further, to the idea that justifications can be violent in themselves. How this might be is quite mysterious, especially since Waldenfels sensibly rejects Lyotard's absurd conflation of coercion and reasoned argument.

While few writers would join Cotta in his appeal to natural law, most would share his general goal of fixing on a distinction between justifiable force and objectionable violence, in preference to Waldenfels' sanguine thesis that violence is absolutely everywhere. The general view is that violence is often difficult to separate from legitimate force but that some such distinction is surely needed. To this end, attempts are often made to argue that certain kinds of force or violence are intrinsically worse than others. In Brady and Garver's collection, for example, John Ladd distinguishes "collective" from "individual" violence as especially offensive. But the species most often singled out for special attention in this way is terrorism, the focus throughout of Frey and Morris's *Violence, Terrorism, and Justice*. One of their contributors, Loren Lomasky, articulates what is probably the mainstream of public opinion in the West when he argues that the mark of terrorist violence is the rejection of civilized constraints. Terrorism is unjustifiable, it seems, by definition.

The trouble with this sort of approach, as Virginia Held points out (her excellent paper appears in both collections), is that it rules out by definitional fiat a question that still seems to make sense, namely, whether terrorism might be justified in some circumstances. As Alan Ryan shows (in Frey and Morris's collection), a broadly utilitarian perspective implies that terrorism may indeed be justified if the anticipated benefits are great enough in relation to the costs. From such a perspective, there is nothing *intrinsically* evil about terrorism; terrorism is merely an extreme and especially frightening application of force. To resolve whether terrorism might be justified, we need to look beyond the concept itself to its consequences.

Here Ryan makes a methodological point that suggests an important criticism of many contributions to the field: "It is not enough merely to stare at such terms as 'force', 'violence', and 'terror', as if we might divine their essences." Too many writers in both of these collections give the impression that conceptual analysis is sufficient—that once we have isolated the salient features of "terrorism," for example, we will automatically know what judgments to make about it. In fact, identifying the features of terrorism is no more than a start; it remains to be explained why those features should be judged in one way rather than another.

When it comes to judging violence and terrorism, we should recognize that the evaluative perspectives open to us now (given the complexity of our moral culture) have the appearance, at the very least, of plurality. Thus, Frey and Morris note in their introduction that ethical consequentialists are more likely to justify violence than are Kantians and that the latter will be less inclined than contractarians to argue that acts of violence place the perpetrators beyond the moral pale. In the Brady and Garver collection, the essays most sensitive to value pluralism are those by Elizabeth Wolgast and Garver himself, the latter drawing on the work of Isaiah Berlin. (On the relevance to this field of historical and political perspectives, as well as moral–theoretical, see Martin Warner and Roger Crisp's edited collection *Terrorism, Protest, and Power* (1990).) These contributions apart, the question of perspective is little discussed in either of these collections. That is a pity, since it is arguably more fundamental than any other. Still, the two books contain much that is stimulating and original; and both make important contributions to the literature.

University of California, Berkeley GEORGE CROWDER

Tragedy and Denial: The Politics of Difference in Western Political Thought. By Michael Brint. Boulder: Westview, 1991. 181p. $38.95 cloth, $12.95 paper.

Michael Brint's first book is at once highly ambitious and curiously modest. It is ambitious in that it proposes nothing less than a new way of reading the Western tradition of political thought, as a succession of *antitragic narratives*, and discusses a wide range of theorists: Plato, Rousseau, Constant, Hegel, Sartre, Derrida, Arendt, and Rorty. This is a daunting list for a mere 168 pages. The modesty of Brint's book lies not in its length, however, but in its pedagogical purpose: it is clearly designed to offer relative newcomers to the field of political theory a narrative "hook" to facilitate their grasp of a large and unwieldy canon. If, as Brint puts it, our tradition of political thought offers "some of the greatest stories ever told" (p. 1), his own work is similarly narrative in character. Like Voegelin, Arendt, or Strauss, he wants to provide a story about Western political theory that uncovers its driving force. Brint's story, in keeping with the book's pedagogical task, is a good deal simpler than these more familiar narrativizations of the tradition. Despite the simplicity of his story, however, Brint clearly intends his work as a contribution to current debates about political theory and the "politics of difference." In my view, *Tragedy and Denial* is more effective as pedagogy—as an "edifying story"—than as an argumentative intervention.

Brint's central thesis is straightforward. He argues that human existence is beset by irreconcilable ("tragic") conflicts between the goods and values of competing life spheres. A "tragic" conflict is, he points out, one between not good and evil but good and good (e.g., the private good of the family represented by Antigone vs. the public good of the state represented by Creon). Such conflicts, Brint argues, constitute an "essential fact" of life, one that political theory has had a difficult time acknowledging. Indeed, Brint suggests that we view the Western tradition of political thought as a series of attempts aimed at "banishing tragedy itself from the human domain" (p. 2). The "denial" referred to in the title is Western political theory's denial of the "essential fact" of tragedy, a denial that ignores "the significance of relevant moral differences" (p. 2). Its "desire to eliminate human tragedy" leads Western political theory to efface the reality of difference, and tragic conflict, by a series of antitragic narratives. In Brint's account, political theory is a succession of stories about political community, stories that hold out the possibility of overcoming or sublating the ethical differences that produce tragic conflict. Brint's paradigmatic examples of such antitragic narratives are, unsurprisingly, the political theories of Plato and Rousseau (two theorists who had little use for difference).

The theoretical point of this perspective on the tradition is also clear. In picturing Western political thought as fearful of tragedy and difference, as seeking an escape from the irreducible plurality of values and spheres of life, Brint echoes a pervasive theme of poststructuralist criticism. Like the poststructuralists, he views the tradition as animated by the desire to efface difference in its most fundamental and intractable forms. Also like them, he makes a plea for preserving and respecting difference as a constitutive fact of our humanity and moral–political life (pp. 3, 11, 164). Brint's vision of the "politics of difference," however, is rooted in an "ethical pluralism" that clearly has a lot more in common with liberalism than with poststructuralism or identity politics (p. 6). Indeed, Brint can be seen as representative of that small but growing species, the liberal who has read Derrida (a species that originates with Rorty).

Within this broad framework, Brint offers a set of readings that constitute the heart of his text. These readings are divided between "pure" exemplars of the antitragic impulse (Plato and Rousseau) and an eclectic group of modern theorists who, according to Brint, attempt to resolve the conflict between individual liberty and democratic freedom through three narrative strategies: comedy (Hegel), satire (Sartre and Derrida), and romance (Arendt and Rorty).

The readings, while intelligent and well informed, are uneven. Brint makes a strong and subtle case for the antitragic impulse behind the *Republic* and Rousseau's political theory; but few readers will be surprised to learn that Plato (or, as Brint would have it, Socrates) and Rousseau were hostile to ethical pluralism. Brint raises a number of intriguing issues (e.g., the narrative structure of the *Republic*, the Socratic inversion of Homeric morality, how irony makes Plato's text a self-deconstructing artifact, the question of Rousseau's subjectivism and theoretical narcissism); but these have all been treated in greater depth by Voegelin, Arendt, and Starobinski. While the treatments here are not exactly original, they are nevertheless concise and thought-provoking; and they are fairly free of plotted "plot summary." The same cannot be said, however, of the readings in part 2, "The Politics of Tragedy." Here, one theorist after another steps on stage just long enough to be typecast (as comedic, satiric, or romantic) and sent packing. Brint's point is to reveal the more insidious forms taken by the antitragic impulse as the threshold of modernity and postmodernity is crossed. This threshold is marked, as Lyotard has claimed, by "incredulity toward metanarratives." While Brint does not mention Lyotard, his point is similar: Hegel's "comedy" of reconciliation was political theory's last "big" story, a story succeeded by Sartre's and Derrida's parodies of metaphysics and, finally, by the reemergence of political theory as "romance,"—as a storytelling that knows itself to be such (Arendt, Rorty). The problem is that the readings in this section, designed to support Brint's narrative typology, are necessarily in broad strokes and not very illuminating. But the reader can hardly expect an original interpretation of a major theorist in 6–10 pages.

Brint does have a point in rushing so many theorists on and off his stage. He wants to show how political theory, even when it takes up difference as its explicit theme and becomes self-consciously narrative in mode, nevertheless continues to evade the full force of tragic conflict. As narrative genres, comedy, satire, and romance evince the same "failure of political nerve" (p. 2), the same antitragic impulse, found in Plato and Rousseau. Brint's solution is to uphold tragedy as a normative model for political theory and to suggest (as the title of his last chapter phrases it) that we "put the 'e' back into *differance*." By this he means that we should break with dialectical or deconstructive treatments of otherness, treatments that in his view thematize difference only to sublate it or reify it into an "ineffable other." In other words, we should avoid the Hegelian temptation of reading difference back into sameness while at the same time resisting the Derridean temptation of simply inverting the "metaphysical" valorization of identity and difference.

I do not find Brint's alternative particularly persuasive. The moral–political thrust of his anti-Hegelian, antideconstructive stance is a plea for a renewed appreciation of the inner articulation of our pluralist polity, an articulation born of different "life spheres," of different "vocabularies, traditions, activities, and practices" (p. 165). Like Rorty and Walzer, Brint wants us to respect these differences and to resist seeking some standard of the just or the good that pretends to render the incommensurable commensurable. This is fine, as far as it goes; but it is (Brint's protests notwithstanding) simply good liberalism. There is no dishonor here: one of the virtues of Brint's book is that it clarifies the pluralist ethical vision of liberalism in a compelling way. He does this, as did Weber, by stressing the pathos, the tragic quality, of conflicts between rival values and goods. Despite the poststructuralist trappings of Brint's book, the result is not so much a *critique* of liberalism as a *restatement*.

Amherst College DANA VILLA

Action into Nature: An Essay on the Meaning of Technology. By Barry Cooper. Notre Dame: University of Notre Dame Press, 1991. 291p. $34.95.

Critical Theory of Technology. By Andrew Feenberg. New York: Oxford University Press, 1991. 235p. $35.00 cloth, $14.95 paper.

Barry Cooper and Andrew Feenberg are both dismayed by technology's assault on nature and human nature. Feenberg allows for minuscule openings in the deadly machinery of modernity while Cooper leaves us with a dispassionate so-it-goes ghetto journal, recording in depth that death is in season, implying (as Andy Warhol did) that one's talent can no longer be employed for anything but embroidering and inflating the products and fetishes of our era.

The world, as Cooper knows it, is in shambles. Its population has been dehumanized by Galilean mathematization, social science positivism, the triumph of light over the mysteries of darkness, and music that is rocking and rolling history to its end. Communists, liberals, fascists are all technicists using means which, in the purposeless whirl of modernity, are themselves ends. "All things," he echoes Marx, "are in movement . . . all is process and change" (p. 102). Global decomposition which Feenberg traces to the reification of ends, Cooper (*pace* Norman Mailer) attributes to the cancer of technicism. And yet Cooper's erudite trouble shooting and whistle blowing—his unrelieved complaints—are not a prophet's jeremiad—for however frequently he recalls Strauss and Voegelin, Cooper never interrupts himself to project a transhistorical order that might give credence to an indictment. He relates; he doesn't blame.

His narrative is a losely concatenated flow of declarations and pronouncements: "Today reality is sufficiently grotesque to make satire impossible: after all, how could it be exaggerated?" (p. 112). "Modern technological societies are not threatened by outbursts of revolutionary compassion" (p. 122). "Now that prosperity has been attained by nearly the whole population, any serious notion of the purpose of leisure has evaporated" (p. 250). And so on.

Cooper makes no claim that alterations are feasible. In the spirit of Heidegger, he envisages no basis for restoring a pre-modernist consciousness. As modest as one of Beckett's characters, he nonetheless goes on—goes on writing and quoting and footnoting while he knows that the ground for going on has eroded. Tenacious, dead-serious, and gravely scholarly, it is a strangely reassuring performance: a case against suicide.

Feenberg's case, on the other hand, is explicitly one for healing the body politic, for recovering the marginalized abiguities of Marx so as to fulfill the promise of socialism. Implictly it is one for bypassing postmodernists whose gay science leads them to embellish industrial products, celebrate fast food, universalize irony, and deconstruct metanarratives all because they assume that it's far too late to reverse an increasingly autonomous technicism.

Feenberg does not feel, as Heidegger finally did, that "only a god can save us now," or that only a state apparatus can impose a new order, or that a simplified lifestyle will heal us, or that a consciousness-raising catastrophy will inspire an immortal phoenix to rise out of the ashes. It remains possible to weave repressed human needs into the stainless steel fabric of the dominant technology. Designed by variable cultures which harbor various desires, technology is neither autonomous nor value-neutral.

Only a metaphysical pathos (in Alvin Gouldner's phrase) of the elites of contemporary society stands in the way of transforming hierarchies of power, one-dimensional work by de-skilled workers, and meaningless consumption. The degradation of labor, liberal education, and the environment inheres not in technology but in the belief system that currently defines technological development.

Yet change can be generated by social movements guided not by Marx's pathetic determinism but by his open-ended vision of a process which can itself arouse and incorporate the latent "irrational" and "inefficient" impulses within individuals and nature. Thus the locus of technical control can be shifted—as in Feenberg's view it might have been in the 1960s—from a control-oriented technicism to a multitude of local social institutions and workplaces.

When Feenberg proceeds to provide examples of the impact of countercultural movements on the capitalist-military-corporate-media complex, he is poignantly evasive. It is disheartening to realize that he keeps himself from sharing his knowledge of the capacity of the technological imperium either to repair (or to embroider) the cracks in the hegemony of capitalism. Sadly, he decides to remain silent about how effectively *Rambo* incorporated the traits of the counterculture in the ideology and policies of the Reagan Era. Feenberg has reason to shy from discussing the limited effects of well-intentioned movements to stop environmental degradation, to free the world (and one's neighbors) from the threat of weapons, or, as in the 1960s, to transform society, its politics, its educational practices, its racism, its compulsions, its very epistemology. Contrary to Cooper, a sad necessity restrains Feenberg.

The reader who spends a month in Feenberg's and Cooper's company may finally come to realize that their books, and perhaps all artifacts patiently lived with, finally complement one another.

University of Hawaii Henry S. Kariel

David Hume and the Problem of Reason: Recovering the Human Sciences. By John W. Danford. New Haven: Yale University Press, 1990. 228p. $25.00.

With the possible exception of Kant and Mill, no modern political philosopher has had as much affect on all facets of current political theory as David Hume. Before I read this book, I was sure that Hume's story in relation to political science had been told; but now I am no longer certain.

The purpose of the book is to establish a role for reason, truth and the use of universal principle in the interpretation and reading of Hume's political thought. Danford asserts that we are now enmeshed in a "crisis of Western rationalism" but that Hume's political philosophy can be utilized to reestablish "the status of reason" (pp. 7–8) in political life. It is obvious to anyone who reads the literature in our field that there is a general skepticism about the role of reason and truth in our studies. Rawls, for example, doubts that metaphysics or truth have any place in "political" concepts of justice, while in other disciplines deconstructionism and radical relativism have built upon the ruins of positivism and reinforced the idea that our task as theorists is indeed a limited one.

This book contends that a reading of Hume that moves away from dependence on the *Treatise* and toward reliance on *The History of England* and the *Dialogues Concerning Natural Religion* will produce a definition of reason and truth that is connected to experience and "common life" (p. 8). The author claims that "common life" acts as a basis of a political theory combining history, religion, and ethics into a bridge that connects the search for reason and truth with an "understanding of what is good for mankind" (p. 193). In this way, the application of reason to the search for truth departs from its dependence on the methods of natural science and moves to the specific framework of political life that defines its proper context. Here, Hume's approach is separated from those who established the use of scientific method in political theory (e.g., Hobbes) and becomes a counterargument to those who have since maintained that "relativism" and "interpretation" must replace "truth" in the theoretical analysis of politics (p. 19).

I had assumed that Hume's political theory rested in the ideas of passion motivating individual action, limited self-interest, convention, and the relative utility of justice and government. This interpretation would make Hume a champion of those who proclaim the death of reason and universal truth in political thought. If Danford is correct, however, the truth of Hume's political theory lies in reason, general principles, common life experience, religion and the historical process of social evolution. Seeking the core of Hume's politics outside the *Treatise*, Danford integrates "common life" with the evolution of individual liberty and the "general principles" of "usefulness and agreeableness" that together provide a framework within which reason and truth in political analysis are reestablished within a stable environment for human social interaction (p. 99).

The argument is elegant and concise (perhaps too concise). However, I am not sure it covers all the bases. Although I am willing to believe that the *Treatise* can be replaced as the core text of Hume's politics, I would maintain that it contains too many supporting beams of what is traditionally thought to underlie Hume's "relativism" to be dismissed without proper argument. I would propose that it is critical for the author to provide a complete transitional argument between old and new interpretations that would overcome the contention that Hume's politics is all about convention and cultural relativism. Danford needs to recast the central role of convention as a competitor for contract, property as a basis of justice/government and the connection between utility and preference that has come to play such a major role in current Hume scholarship (e.g., in both collective action theory and "preference" utilitarianism). Instead, I remain confused by such statements as that Hume rejects convention-based moral theory but maintains an "unblinking acceptance of the actual desires and preferences of mankind" (pp. 157, 160).

This book is one of the most interesting and unique renderings of Hume's epistemology and political and moral thought that I have ever come across. It is a must read for anyone interested in Hume's philosophy and its relation to political science or to anyone who is troubled by the relativism and positivism that provides the framework for most of what passes as social science these days. It is a perfect text for a seminar on Hume; and although I believe that more argument should have been provided as to how this "new" reading of Hume's politics can replace the "traditional approach," the uniqueness and creativity of this book, as well as its pertinence to current theoretical debate, is enough for me to recommend that anyone interested in all the dimensions of Hume's political thought ought to have this volume in their library.

Trinity College, Hartford JOHN MARTIN GILLROY

The Liberal Self: John Stuart Mill's Moral and Political Philosophy. By Wendy Donner. Ithaca: Cornell University Press, 1992. 229p. $35.00 cloth, $12.95 paper.

The Liberal Self proposes to "resolve some traditional puzzles" raised in the secondary literature on John Stuart Mill's thought (p. 2). Most of the book is devoted to a discussion of Mill's moral philosophy. However, since Wendy Donner maintains that "Mill's utilitarian philosophy unifies his ethics and his liberal political philosophy" (p. 1), *The Liberal Self* should be of considerable interest to those concerned with Mill's political thought. This interest may be tempered by Donner's interpretation of Mill's view of political theory.

According to Donner, Mill's system of ethics is grounded upon the preferences of knowledgeable agents, individuals who have developed their intellectual and moral capacities (p. 5). At the same time, she argues, "Mill's most fundamental commitment . . . is the promotion of human self-development and the happiness involved in the development and exercise of our higher human faculties" (p. 3). On the face of it, there appears to be an irreconcilable tension between the absoluteness of a universal right to self-development and the preferences of a privileged minority who have developed their capacities as an axis from which to evaluate competing moral claims. Nevertheless, Donner believes that Mill's "sophisticated form of hedonism," in which qualitative judgments supply the definition and measurement of value, can be defended as an objective and logically consistent expression of utilitarian moral theory (pp. 4–5, 9, 55).

Few readers are likely to be convinced by Donner's defense of qualitative hedonism. In this and in other instances throughout *The Liberal Self*, the problems she draws from the secondary literature on Mill are more profound than her responses to them. Still, she does have some valuable things to say regarding the social basis of rights in Mill's thought; and she quite rightly stresses the importance of self-development to Mill's moral and political theory. But to affirm truly that Mill is equally concerned with sociality as he is with individuality or that he defends a positive, as well as a negative, conception of liberty (pp. 148, 162) hardly constitutes a resolution to the puzzles various critics have discovered in Mill's thought. Indeed, to admit that Mill does not tell us how to employ the principle of utility to resolve a conflict of vital interests (as Donner does, p. 187), leaves us inhabiting a world characterized by social conflict where appeals to such vacuous propositions are not likely to provide any guidelines for practical action.

However, whatever the moral dilemmas confronted by individuals qua individuals, Mill does offer a specific political example of what would count as a successful application of utilitarian philosophy. "The test of Mill's commitment" to the goal of human self-development, Donner notes, "is his stance on opportunities of development of the working class of nineteenth-century Britain" (p. 198). To appreciate the extent of Mill's "commitment" one should include (Donner surprisingly does not) the self-development of women. Still, if we consider this test as applied to the working class, both the tensions internal to Mill's thought and the difficulties with respect to Donner's interpretation of it will become apparent.

Assuming that Mill's political theory is simply the logical extension and practical application of his moral philosophy, Donner maintains that Mill's political thought expresses the preferences of knowledgeable agents (pp. 131, 202). To grasp the implausibility of such a view, one need only consider Mill's commitment to democracy and universal suffrage in the context of nineteenth-century British politics. Such a perspective was not espoused by Mill's intellectual contemporaries, the vast majority of whom sided with Macaulay in opposing democracy. In other words, it was the working class, not a developed or knowledgeable elite, who advocated political and economic democracy—the core of Mill's political thought. Moreover, Mill did not believe that political theory could be identified with the preferences of knowledgeable agents. Rather, following Alexis de Tocqueville, he maintained that observable empirical sociological tendencies at work in society provided the foundation for any political theory. The political theorist's task, Mill argued, was to ascertain the good and bad effects of these tendencies and to assist the extension of the former and inhibit the development of the latter. Not only was it possible for any perceptive observer, including members of the working class, to grasp the significance of the empirical tendencies favoring democracy or cooperative socialism, for example, but it was also possible for highly educated intellectuals to pay insufficient attention to such sociological evidence and thus to hold a seriously deficient political theory.

Donner maintains that Mill's elitism makes him "overly protective of the interests of the middle class at the expense of working class advancement." Thus, she asserts, "Mill assumes that the already competent members of the middle class who are to help others become competent are able to put aside their class interests and make social choices grounded on the interests of all" (p. 202). This is patently absurd, as any reader of *Considerations on Representative Government* knows. In that work, Mill argues for equal representation in Parliament of middle- and working-class interests precisely, he insists, because neither class is capable of "putting aside" its class interests. A tiny number of individuals from *both* classes, Mill suggests, will, with respect to a particular legislative proposal, be able to see beyond their class interests, thereby securing its passage (the majority of members on both sides voting in accordance with their class interests).

According to Donner, Mill is concerned only with the dangers posed by the working class's coming to power but not with the abuse of political power by the middle class (p. 203). But based on Mill's correspondence and his published writings, not only is it Mill's view that class-interested abuse of power is a danger in all forms of government (not merely a defect peculiar to democracy or to the empowerment of the working class), he also has some especially harsh criticisms to make of the British middle class, which, he argues, has already demonstrated its unfitness to rule. Far from being concerned with protecting a particular class's interest, the problem for Mill was to find a way to put an end to class conflict.

It is ironic that Donner accuses Mill of having little sociological knowledge of the workings of his society (p. 207). On the contrary, it is Donner who presumes that social, political, and historical context are useful merely as background information. The fact is that the philosophical problems as they appeared to Mill cannot be formulated correctly in the absence of such contextual knowledge.

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Regarding Politics: Essays on Political Theory, Stability, and Change. By Harry Eckstein. Berkeley: University of California Press, 1992. 404p. $45.00 cloth, $17.00 paper.

This collection of Harry Eckstein's monographs and articles, reprinted from journals and books which have appeared over the last quarter century, is almost, but not quite, representative of the rich and varied body of his work. It includes three monographic essays which ensure Eckstein's membership in the small group of scholars who have made creative contributions to the definition and methodology of political science. Had he not reprinted them here, the obsolescence of the symposia and handbook in which they originally appeared, might have imposed prohibitive "search" costs for a contemporary scholarly generation indifferent to disciplinary history. His essay "Political Science and Public Policy" is still one of the best interpretations of Max Weber on these themes. At a time when the termination of the Cold War enables us to reconsider the politicization of our scholarship, Harry Eckstein's 1966 paper analysing Weber's writings on these topics and political activities during the late Wilhelminian Empire and the early Weimar Republic belongs at the top of the reading lists of our syllabi for introductions to political science. His "Historical Perspective on Comparative Politics," introducing the anthology that he and David Apter published in 1963, still has no rival in scope and erudition. His "Case Study and Theory in Political Science" is one of the most original methodological contributions of our time, bringing case studies into the world of scientific rigor and theory and turning the ideographic/nomothetic dichotomy into a continuum with five distinguishable degrees of theoretic–scientific sophistication. Drawing on work of Sidney Verba but pressing the argument to its full implications, Eckstein spells out the properties of the "ideographic–configurative" case study, which makes no claims to theory, at one extreme and the "crucial-theory-infirming," or "theory-confirming," extreme at the other. The three types in between—"the disciplined–configurative," the "heuristic," and the "plausibility probe"—take increasingly firm steps in the direction of self-conscious hypothesis testing and theory building.

Eckstein is a strong advocate of the crucial case study as the way to theory. While he does not denigrate statistically based theory, he makes a convincing case for the special value of clinical depth in avoiding the loss of important information. It has been an only partially fulfilled aspiration of Eckstein's to conduct a crucial case study of his "congruence theory" of political stability (his study of Norway); and he has not as yet done the study that might settle the rivalry between the political culture and rational choice approaches to explanation. Eckstein takes political science very seriously indeed. His model for crucial case studies in political science is the series of natural observations that have confirmed (or failed to falsify) Einsteinian relativity. Michel's theory of political oligarchy based on the study of the organization of the Social Democratic party of Germany is Eckstein's strongest example of a crucial case study in political science. While a program of accumulating a large collection of crucial case studies in political science may turn out to be visionary, Eckstein's exhortation to carry out case studies with theoretical insight and discipline and his systematic elaboration of the various ways this can be done is wholly constructive and admirable.

Creative scholars are risk-takers; they have the courage to propose new approaches and theories even though they may fail. In the early 1970s Eckstein made the bold proposal that political scientists redefine the focus of their discipline from the state and government to authority relations in general (family, church, school, economic enterprises, associations, etc., as well as government and the state). This initiative had its origin in Eckstein's congruence theory of political stability, that is, the hypothesis that a political system with organizational characteristics similar to the main nongovernmental structures would be stable, whatever that organizational structure might be. His theory of stable democracy—now more than three decades old and republished here as part 3, "Political Stability"—added to the organizational congruence requirement for generic stability the "balanced disparity" requirement for governmental–political institutions. In other words, democratic stability would require not only organizational congruence but limits on democracy. Such are accomodativeness to authority; deferential attitudes, as well as egalitarian and achievement values; and consensualism, as well as conflict. Having formulated this theory in the late 1950s on the basis of an informal comparison of England, France, and Germany, he carried out what approximates a "crucial case study" of Norway in the mid-1960s. His essay on case studies came after his theory of democracy had been formulated and tested in the case of Norway—a country about which Eckstein at the outset knew little save that it was a good democratic performer on all counts and stable in all aspects. His book, *Division and Cohesion in Democracy* showed that that country had both organizational congruence and balanced disparities.

Eckstein's theory of political stability was the high point of a research program that had begun most auspiciously in his early work on the British Medical Association, on interest groups, and on empirical pluralist theory. A member of the scholarly generation that lived through (in Eckstein's case most personally) the traumatic years of the fall of the Weimar Republic, the crisis and fall of the Third Republic, the rise of fascism and nazism, and World War II, he shared the preoccupation with the conditions and characteristics of stable democracy. Of all of that generation, his work on this theme was the most original and the most seriously theoretical.

Having gained such leverage on so important a problem as democratic stability by emphasizing general social organizational and cultural characteristics, Eckstein took what seemed to him the logical next step: he proposed that political science as a discipline change its focus from government and the state to authority relations, broadly speaking. If nongovernmental structure and culture were of such importance, then why not broaden the field and thereby the body of explanatory knowledge? Drawing the consequences of this argument, he, along with his colleague Ted Robert Gurr, embarked on a research program that culminated in a book entitled *Patterns of Authority*, which invited the political science profession to adopt this inclusive paradigm in the place of those then dominating the field.

In his introduction to *Regarding Politics*, Eckstein refers to his effort to broaden the subject matter of political science as the "greatest failure of his career." Let me console him with the thought that our colleagues are much more indulgent of us than this passage in his introduction suggests. The response to his appeal, judging by the productivity of Eckstein's students in nongovernmental authority studies, was quite forthcoming and was enriching of the literature on political socialization and political culture. There are, indeed, few restrictions on the kinds of authority patterns that political scientists are encouraged to investigate. If there was any fault in Eckstein's appeal to broaden the focus of political science, it was that the walls were already largely down. It may be that the packaging of his book on authority diverted attention from its rich and suggestive content and that future generations of scholars will draw on it for research designs and for hypotheses.

Parts 4 and 5 include articles and papers on political development, political culture, political violence, and, most intriguing as indicative of future Eckstein work, "Civic Inclusion and Its Discontents." Here he is pursuing themes first developed in Tocqueville's *Democracy in America*, particularly the second volume forecasting the spread of equality and all the promise and problematics that this trend implied. As one would expect from his prior work, Eckstein's notion of the scope of civic inclusion means much more than the spread of the suffrage and formal access to officeholding. He is concerned with family, peer group, neighborhood, school, and workplace participation and acculturation, since this creates the culture of the poor and shapes and limits the process of democratization. This essay promises another important contribution from the Eckstein atelier.

Stanford University GABRIEL A. ALMOND

Interpersonal Comparisons of Well-Being. Edited by Jon Elster and John E. Roemer. New York: Cambridge University Press, 1991. 400p. $49.50.

In view of the contemporary opinion regarding the intractability of the problem of interpersonal comparisons of well-being, it may seem curious that the classical utilitarians (Bentham, the younger Mill, and Sidgwick) were scarce troubled by this issue. To be sure, they were aware of the considerable practical problems of measurement; of the propensity of individuals to make mistakes in hedonic judgments even with regard to themselves (let alone others); of the want of reliable scientific understanding of the causes of pleasures and pains, especially in regard to quantity; of the diversity of individual satisfactions and displeasures; and of the great uncertainty about effects, under which decisions about what would be conducive to the greatest happiness would have to be taken. Nonetheless, all three were in agreement that the thing to be measured was an objective concept amenable to scientific enquiry (Mill's idea about higher and lower pleasures notwithstanding). In Bentham's and Sidgwick's accounts of this, it almost seems as if measuring pleasures and pains, and thus well-being, was akin to measuring blood pressure or body temperature. (Sidgwick talks of "psychophysics," i.e., physiological psychology, in this connection; see *Methods of Ethics* 2.6.) Mill's account of the test for quality of pleasure in chapter 2 of *Utilitarianism* is all too brief; but it seems clear from what he does say about the matter that the "verdict of the only competent judges" was meant to be an objective one, good for all. If so, then it is understandable why no one among them might have seen interpersonal comparisons of such a measure of well-being as being in the least problematic.

On the other hand, once we follow the path of the economists from the classical hedonic concept or concepts to the modern concept of utility as subjective preference, to be revealed by the choices individuals are disposed to make, then the problem of interpersonal comparisons does, indeed, loom large. In particular, part of the skepticism about John Harsanyi's reconstruction of utilitarianism derives from doubts on this score.

Of the 11 papers in this volume, 6 are of a technical nature, from which the reader is not likely to get much without at least some acquaintence with the mathematics used in social choice theory or welfare economics. The other five, however, should be more generally accessible.

To begin with the skepticism about Harsanyi and one of the technical essays, Amartya Sen some years ago raised the question of whether the theorems at the core of Harsanyi's defense of his preference utilitarianism were substantive proof of his position or were (as Sen argued) merely theorems about the representability of social preference that are consistent with a nonutilitarian interpretation. In short, what has Harsanyi really proved? John Weymark, in his contribution to the volume, gives a meticulous analysis of this question, a model of the clear exposition of a technical matter. The verdict is that Sen was right.

Both Tim Scanlon and James Griffin, in their chapters, argue that we must move in the direction of a more "objective" measure of well-being if we are to cope with the interpersonal comparison (though for somewhat different reasons). Neither advocates a return to Bentham or Sidgwick, however. Scanlon argues for a more indirect measure along the lines of Rawlsian social primary goods, and Griffin would have us move in the direction of Mill. Both argue that the proper choice of concept of well-being is a substanive issue of ethical theory, an insistence found in several other contributions, as well (e.g., Weymark's and Hammond's). Scanlon's analysis provides a critique of Harsanyi's assumptions about the interpretation of individual preference that is complementary to the discussion given by Weymark.

Daniel Kahneman and Carol Varey, in their essay, present interesting evidence from the psychological literature to the effect that individual preferences and hedonic experience (their term) may diverge significantly, especially where the former involves anticipation of future experience or the latter, adaptation or context effects of various sorts. The preference model, in practice, would diverge significantly from one along the lines of Bentham and Sidgwick; thus, this is not an idle distinction. On the other hand, the evidence about adaptation or context effects suggests that the Benthamite project is rather more complicated than the opening remarks about Bentham or Sidgwick would suggest. It is unfortunate, however, that the evidence they present does not appear to give us much of a handle on the issues raised by Griffin and Scanlon.

But supposing that we follow Griffin, in the direction of Mill, is this approach technically viable? Suppose our "only competent judges" are only "local" experts (but that any two agree on matters of mutual experience). Ortunõ-Ortin and Roemer show that we can piece together these local judgments to get a global interpersonal ordering. This is the good news. The bad news is that if we allow everyone to be a "global expert," do not require agreement, and impose analogues of the Arrow conditions, then we get an analogue of the Arrow result in the form of a "judgment dictatorship"—as Aanund Hylland shows in his chapter. Weaken the universal domain condition to require "antipaternalism," and the conditions become consistent with a "positional dictatorship" (e.g., the maximin rule). But these results ought to be expected, I think, given that Hylland restricts attention to ordinal preferences and ordinal interpersonal comparisons.

Peter Hammond's chapter gives a broad survey and appraisal (mostly negative) of the ideas for interpersonal comparisons that have been tried in welfare economics and includes an extensive (perhaps exhaustive) bibliography. His suggestion at the end that a successful model of interpersonal comparisons will involve judgments about what sorts of people are desirable seems alarming at first blush but may be somewhat less troubling (though still provocative) once one realizes that we already are comfortable with some such judgments (the undesirability of sociopaths, to take an extreme case). The technicalities are at a minimum here, and Hammond's chapter should be accessible to a wider audience than just those skilled in mathematical welfare economics. The same, alas, cannot be said of the chapter by Blackorby and Donaldson, "Adult Equivalence Scales."

The other chapter that I found instructive is that by Jon Elster about the ingredients that seem to go into small-scale (*local* is Elster's term) policy decisions involving interpersonal comparisons of well-being. Elster has space for detail about only a small subset of the kinds of policy decision he initially presents (from allocation of scare medical resources like kidney transplants to allocation of tasks within households), but there is enough to show that "it is a messy business"; and even when comparisons of well-being are the primary feature of a policy decision, significantly different aggregation procedures may implicitly be used, depending on the nature of the decision. Elster has useful things to offer by way of bringing some order into this picture, and his chapter is a welcome addition to a collection primarily of a more theoretical character.

In summary, the primary audience for this book will be those doing normative political theory, political philosophy, political economy, or the like, with some technical background in social choice theory or welfare economics and a formal bent. They will, I think, find much that is of interest here. Some of the material should be accessible, and of interest, to philosophers, social scientists, and policy analysts for whose work the issue of interpersonal comparisons of well-being is pertinent. Finally, a number of interesting problems for further research both formal and empirical are to be found herein.

University of Texas, Austin PAUL LYON

Democracy and Deliberation: New Directions for Democratic Reform. By James S. Fishkin. New Haven: Yale University Press, 1992. 133p. $17.95.

This small book ranges widely over topics of interest to students of democratic political systems—from the appropriateness of democracy for the governing of large nation–states to problems confronting peoples seeking to move from authoritarian to democratic rule. Its centerpieces, however, are a critique of recent democratic reform movements and a proposal for remedying some shortcomings of the present procedures for nominating presidential candidates in the United States.

Fishkin argues that a fully realized democratic system (to qualify as such) must weigh its citizens' preferences equally in political decisions, insulate those preferences from influences that threaten their integrity, give interests with significant followings effective opportunities to be heard, reject policies that destroy anyone's essential human interests when policies without that result could be chosen, and permit citizens equal and sufficient opportunity to discover and test their preferences on issues to be decided. The actual institutions and procedures of democratic states satisfy these conditions (which Fishkin subsumes under the headings of political equality, nontyranny, and deliberation) very imperfectly; and arrangements that further the realization of one may hinder realization of another. The reforms in the late 1960s of the U.S. system for nominating presidential candidates are a striking illustration of this kind of trade-off. They gave many more people a direct say in these important choices but made the process of deliberation about them a largely empty one.

Fishkin proposes that we choose a sizable body of citizens of voting age by random sampling methods; bring these citizens together for several days and before any states have held caucuses or primaries; and assign Democrats and Republicans to separate "national caucuses," letting independents choose which of these they will join. Then, Fishkin suggests, we should bring presidential candidates before the appropriate caucus, singly and in debates, and let its members question them about themselves and their positions on issues. After the members of each caucus have debated the issues with candidates and each other, we should poll them to learn their choices of candidates and opinions on the issues and (ideally) let them choose a limited number of delegates-at-large. Broadcast some part of these proceedings on national television.

This ingenious proposal has already drawn much attention; indeed, a version of it came near to realization this year. Its intent is to offer "a means of launching candidacies and issues in a competition that is more representative of the entire country than either Iowa or New Hampshire and more deliberative than our conventional campaign process" (p. 4). On some reasonable assumptions, the proposal is a very appealing one. These assumptions are that (1) the major parties will continue to choose most of the delegates to their national conventions in sequential primaries; (2) the sample of voters drawn will in fact be representative of eligible voters nationally; (3) members of the two national caucuses will be able to inform themselves at least as well as those voters who participate in the Iowa caucuses and the New Hampshire primaries now do; and (4) news agencies, contributors, volunteers, and primary voters will see the national caucuses as important tests of the viability of candidacies. Grant these assumptions, and you have gone far toward agreeing that Fishkin is proposing—if not a better way to run the railroad—a better way, at least, to start the train down the track.

Fishkin's agenda for thought and action goes considerably beyond this proposal for national caucuses. These are, in fact, only an example of what he calls a "deliberative opinion poll," that is, a poll that "models what the electorate *would* think if, hypothetically, it could be immersed in intensive deliberative processes" (p. 81). Such polls, though their high costs would necessarily limit possible applications, might play a useful role even in a system for nominating presidential candidates like that abandoned after 1968 or in the consideration of issues submitted in referenda. In any case, deliberative opinion polls are in turn but one example of what Fishkin truly has in his sights, namely, reforms of the institutional processes of nation–states that will realize both political equality and deliberation.

The strength of this book is its capacity for provoking thought about the institutions of representative democracy and ways to make them serve basic democratic values. It also has some weaknesses. It is sketchy in its presentation of arguments, and some claims are exaggerated or otherwise questionable. For example, even critics of the present system for nominating presidential candidates are likely to balk at the flat characterization of it as "momentum without learning; choice without deliberation" (p. 6). And the claim that the U.S. Congress has moved toward majoritarian control is dubious, at least, however much its members now cultivate the electoral connection. Theorists will find little new theory of a fundamental sort, students of institutions, no new evidence about them. To make much of this last observation, however, would be to miss the point of the book. The author intends not to produce new evidence or to build new theory but to invite us to think about how to bring theory and knowledge to bear on the design of better political institutions. The invitation is both an engaging and challenging one, and he sets a good example.

Princeton University STANLEY KELLEY, JR.

Liberal Purposes: Goods, Virtues, and Diversity in the Liberal State. By William A. Galston. New York: Cambridge University Press, 1991. 343p. $49.50 cloth, $16.95 paper.

Is it possible to do political philosophy without any conception of what is good or valuable in human life? Some have tried: John Rawls and others have explored the possibility of grounding principles of justice and right on procedural ideas like equality, reciprocity, and deliberation. Their critics delight in showing that such an enterprise is bound to fail. Reciprocity and equal respect, they say, remain empty as ideals absent some account of what we value in each others' agency; and deliberative norms cannot yield principles for social life without some account of the substantive point of our conversations.

Those who retain any shred of faith in the proceduralist project should read William Galston's new book, for it is both a clear and up-to-date attack on the proceduralist experiment and a new and sympathetic outline of the value commitments that are actually presupposed by the core positions of liberal political morality.

The critical part of the book is mainly an attack on liberal neutrality, that is, on the principle that both political decisions and normative theories of political decision making should be, in Ronald Dworkin's words, "independent of any particular conception of the good life, or of what gives value to life." Galston considers four justifications for neutrality: skepticism about value, free individual choice, peaceful accommodation among a diversity of groups in society, and equal respect in political argument. The first is easily dismissed: a general skepticism about value can hardly justify a *principle* of neutrality. The fourth, equal respect, elaborated in recent discussions by Charles Larmore and Thomas Nagel, is too constraining for Galston's taste. If we are prohibited from raising anything that offends anyone's cherished beliefs, then most of what we understand as public argument will be ruled out. The middle two are less easy to dispose of but also less obviously proceduralist: each appeals to a conception of the good—either to values concerning autonomy or to the values involved in giving priority to life and peace. So neither can be used as a reason for excluding considerations of the good from politics.

There is nothing strikingly new about this discussion. Galston's account has both the advantage and the disadvantage of being very much up to date. Because it directly addresses the latest articles in the debate about liberalism, it has the character of an informed engagement in the immediate fray, rather than of a more detached discussion of the philosophical issues involved. Neutrality actually raises some very deep questions about why we organize ourselves into states; yet most of the modern discussion takes the existence of the modern state for granted. Thus, Galston does not discuss the "How dare they!" defense of neutrality—the argument that calls in question the very idea that it is for coercive organizations to take stands on matters of ethics, value, and religion.

The second half of the book, in which the author sketches out the value premises of an acceptable theory, is likely to be more important to readers. The crucial chapters are entitled "Liberal Goods" and "Liberal Virtues." We are told that the liberal conception of the good comprises the following values: life, the development of basic capacities, the fulfillment of interests and purposes, freedom, rationality, community, and subjective satisfaction. The virtues liberals should be inculcating are things like courage, loyalty, independence, tolerance, self-restraint, and an ability to listen to another's point of view.

I wish Galston had devoted more space to the detailed discussion of all this. It's not that the lists are wrongheaded. On the contrary, his account is plausible and largely incontestable. However, the philosopher's job is not to pander to our intuitions but to take what sounds plausible and make it seem contestable for sake of bringing out its deeper significance.

Most of the discussion in the book is programmatic. It is about the very idea of liberal goods and virtues, rather than the particular conception itself. It is as though Galston were engaged in the debate about neutrality throughout the book and not just in the first half—as though the goods and virtues were listed in the later chapters purely for the sake of illustrating the more abstract antiproceduralist argument. Surely, if liberalism involves commitment to substantive goods, then liberal political philosophy should consist in the deep and systematic exploration of those goods. In *Liberal Purposes* there are precisely 16 lines devoted to a discussion of the good of life. But whether life (and the peace that secures it) is the preeminent good is precisely the issue on which the whole tradition of liberal toleration turns. There is less than half a page on the satisfaction of desire. But whether satisfaction is worth pursuing for its own sake (even as one good among others) is a crucial issue between liberals and their opponents. Again, there are just 9 lines on the virtue of loyalty. Should one be devoted primarily to the liberal institutions of one's own country, or should one be devoted to their realization everywhere? Are liberal virtues too universalistic to sit easily as civic virtues? These questions are not answered.

We await a discussion that explicitly shows what it is to pursue intimations about the good life in a substantive way if that is what we should be doing. Meanwhile, at the end of this book the most prominent difference between "proceduralist" liberals and their opponents remains that the former, like John Rawls, have actually *done* political philosophy in the style they advocate (whether convincingly or not is another issue), whereas the latter have so far spent all their energy just advocating a change in style.

University of California, Berkeley JEREMY WALDRON

Legal Hermeneutics: History, Theory, and Practice. Edited by Gregory Leyh. Berkeley: University of California Press, 1992. 325p. $15.95.

The word "hermeneutics" tends to divide people into two camps, those who won't go near any book that has the word in the title and those who will buy such a book precisely because it does. For those of you who are in the first camp (but have gotten this far into a book review on the subject), *Legal Hermeneutics*, edited by Gregory Leyh, is a very good place to catch up on what you have been missing, whether you are specifically interested in law or not. For those of you who are already fans of hermeneutics, this book contains a valuable and unusually lucid set of contributions to the development and application of hermeneutic ideas in the law.

What is hermeneutics? Many of the authors, particularly Fred Dallmyer and Gerald Bruns in their excellent essays on the rule of law and the situatedness of texts, respectively, turn to Hans-Georg Gadamer for inspiration. Gadamer believed that interpretation (the activity hermeneutics attempts to understand) is always practical, done for specific purposes in specific situations. Interpreters invariably draw on their broader historical and cultural understandings to work out what various texts mean in the concrete situation at hand—but not because they choose to. Instead, Gadamer argued, they can do otherwise.

These observations have been devastating for many who believe in the rule of law and in the ability of legal texts like the Constitution to govern through time. Many arguments about the dependence of the meaning of legal texts on the historical and cultural contexts of readers have slid directly into claims of radical uncertainty, instability, and illegitimacy of the law. As most of the authors in this anthology make plain, however, this is not where a deeper hermeneutical understanding should lead. Instead, hermeneutics illuminates why it is that readers in common cultural circumstances generally *do* agree on what a text means. It is precisely because these readers share a common way of life (practices that underwrite interpretations that underwrite practices, etc.) that the meaning of cultural products like legal texts is generally widely shared. And, as Ken Kress notes, *some* indeterminancy in legal interpretation does not completely undermine the legitimacy of law anyway.

Legal Hermeneutics brings together some of the most impressive authors writing about this set of issues. Though a few of the essays (those by James Farr, Kress, and Michael Perry) have been published before, the other essays (by Dallmyer, Bruns, Peter Goodrich, Jerry Stone, Terence Ball, Druscilla Cornell, David Cousins Hoy, Steve Knapp and Walter Benn Michaels, Lief Carter, Gregory Leyh, and Stanley Fish) are published for the first time here. Most of these writers have their primary backgrounds in political or literary theory, rather than in law alone. As a result, this is not the usual set of inquiries about how the Constitution ought to be interpreted in specific doctrinal areas currently under contention but is, instead, a much more enduring contribution to how political and literary theory drives us to reconsider the interpretive practices of the political community in which we live. In general, the quality of historical understanding and theoretical sophistication is much higher in this collection than one generally finds in the vast literature on interpretation and the law.

Hermeneutics is seen in this volume as a discipline with a history, not just a set of slogans to be lobbed at current events in the law. Farr writing about Francis Lieber; Goodrich writing about Petrus Ramus and his followers; and Stone writing about Rudolf Bultmann, Karl Barth, and Paul Ricoeur show how hermeneutical inquiry is not a recent invention for the postmodern age of fragmentation (though Cornell writing about Jacques Derrida, Maurice Blanchot, and Emmanuel Levinas does give the problem a decidedly postmodern spin). The subjects of these essays are people who have thought long and hard about how texts change their meanings over time and yet (for all but the latter set) believe that the interpretation of texts remains within a tradition that is never completely lost.

The largest disagreement in this volume is over the relevance of the historical construction of intention in understanding the meaning of a text. While Knapp and Michaels, as well as Fish, have long thought that intention was crucial to interpretation, Ball and Hoy mount forceful arguments against that view. There are not clear winners and losers among these essays, but one does wonder how much it matters that those who defend a concentration on intentionality have literary works as their main examples and those who reject intentionality concentrate primarily on law. As Gadamer would be the first to point out, the conventions of practice within particular communities of interpretation make all the difference.

In the concluding section of the book, Carter, Perry, and Leyh write about the implications of these debates for legal practice. Perry focuses on how judges' choices of theories of interpretation do affect the outcomes of cases. Leyh concentrates on the conventions of legal interpretation taught in law schools. But perhaps the most telling essay in the whole volume is the description by Carter of a week he spent with 14 trial judges. For these judges, legal hermeneutics did not even register on their mental maps.

That said, there remain good reasons for scholars to be interested in these questions. A focus on hermeneutics connects legal interpretation with other interpretive practices, most notably in theology and in literary criticism. By enlarging the scope of inquiry beyond the narrow and professionally policed boundaries of the law, this volume provides a way to reconstruct the historical and cultural context of debates over legal interpretation, which hermeneutics itself should tell us is where all the action is in the struggle over meaning.

University of Michigan KIM LANE SCHEPPELE

Essays on the Foundations of Aristotelian Political Science. Edited by Carnes Lord and David K. O'Connor. Berkeley: University of California Press, 1991. 255p. $45.00.

The essays in this volume are united in their effort "to rectify the methodological and substantive weaknesses in current approaches to Aristotle's practical science" (p. 7). Most of all, they aim to show the preeminently practical intent of Aristotle's political works. The first four essays examine Aristotle's "social science" generally, and his "anthropology," "economics," and "sociology." The remaining five give fuller treatment to some of the themes that inform the foundations of Aristotle's political thought: justice, gender and citizenship, the best form of government, and the rule of law. The contributors are political theorists (Carnes Lord, Waller Newell, Stephen Salkever, and Abram Shulsky), philosophers (Richard Bodéüs and David O'Connor), a classicist (Josiah Ober), and an historian (Barry Strauss).

Lord claims in the Introduction that Anglo-American scholarship approaches Aristotle "at once too historically and not historically enough" (p. 1). The main reason scholars, with the exception of Leo Strauss and several contemporary Germans, tend either to historicize or to abstract Aristotle, Lord explains, is their failure to appreciate that he aspires primarily to improve (all) human action, and only secondarily, if at all, to theorize about phenomena. Being neither strictly parochial nor strictly theoretical, Aristotle's practical orientation to the human sciences challenges the historicism and positivism that prevail in philosophy and the social sciences.

In addition to demonstrating Aristotle's commitment to practice, the essays correct several particular interpretations, such as that Aristotle regards man as fundamentally participatory, the household morally sterile, and justice opposed to self-interest.

The best of the nine essays are the two by the editors. In "Aristotle's Anthropology," Lord argues that Aristotle's understanding of the origins of society differs from that of contemporary anthropology, which regards the family and kinship relations as critical stages. Lord contends that the evolutionary or organic reading of Aristotle's account of the development of the city rests on a misunderstanding of his view of man's politicality;

man is fundamentally not a pairing or bonding animal, according to Aristotle, but a combination of spirit and reason. Spiritedness, not sociality, explains the coming-to-be of society; for it impels men to resist being ruled as much as it brings them together, and it explains barbarism and tells why society did not (and does not) come to be according to a gradual, universal process. Aristotle's focus on the household at the beginning of the *Politics* is a rhetorical move, Lord explains. Aristotle does not want to reinforce either conventionalism or agonism in political life but aims, by establishing the naturalness of political rule, to reinforce the comradeship that arises naturally among men from their common perception of, and desire to secure, the just and noble.

O'Connor's "Aetiology of Justice" should be read by all theorists interested in the subject of justice. Its main objective is to weigh the claims of a contemporary view of justice, a view which links justice with altruism and impartiality, against those of Aristotle's view, which regards orientation of desire as central to justice. Presupposing a distinction between self- and other-regarding virtues, contemporary moral philosophers, such as Bernard Williams, define *injustice* as a kind of egoism, an indifference to promoting just distributions. Contrary to contemporary interpretations of Aristotle (e.g., Philippa Foot's), Aristotle does not, according to O'Connor, distinguish self- from other-regarding virtues; justice is the symptom or relational manifestation of correctly oriented desires. The source of injustice is, then, according to O'Connor's reading of Aristotle, not a *specific* vice, like egoism or indifference, but misoriented desire. The problem fundamental to justice, then, is "not the aetiological one of the possibility of altruism" but the political one of the possibility of a coincidence between the good man and the good citizen (p. 147).

Salkever's opening essay contrasts Aristotle's "social science," which seeks to improve praxis by presenting the human telos not in terms of some transcendental ideal but as the mean, with both empiricist and interpretive social science, both of which preempt moral relevance—empiricism by reducing human beings and their creations to composite parts and the interpretive approach by denying any essential human relation to nature (its findings are strictly local and archival). Such criticism is compelling, but Salkever dissolves the tension between politics and philosophy that Aristotle sees by blurring Aristotelian distinctions between the nature of the activities, their ends, and the faculties engaged.

Shulsky's essay, which argues that economics, for Aristotle, is a practical science, lacks discussion of Book 5 of the *Nicomachean Ethics*, part of which might be understood to be an attempt to theorize about economics that is compatible with his practical counsel.

Ober's piece on Aristotle's "political sociology" argues that Aristotle's categories of sociopolitical analysis converge not with the materialistic Marxist concept of class, which is fused with the concept of exploitation, but with the more ideological, Weberian categories of status and order, and are useful in showing the societal differentiation of the polis that Aristotle himself recommended. The contemporary relevance of the *Politics*, then, with its presentation of an ideal ordered state with a homogeneous citizenry, is its attempt to show how the political and social can be linked to a socially diverse world so as to shore up moral community as far as possible.

Salkever's second and more successful contribution (published, like his first, in two previous versions) argues that Plato's and Aristotle's countercultural reservations about how far political activity can make human beings virtuous go hand in hand with their appreciation of things private, such as the family and women.

Newell, Strauss, and Bodéüs write on Aristotle on regimes. Newell points out that Aristotle's association of monarchy with absolute justice reveals both monarchy's status as a theoretical standard and political community's nature as a creature of compromise. Strauss claims that Aristotle intentionally—for socioeconomic, political, rhetorical, and philosophical reasons—depicts Athens as an extreme, rather than as the constitutional democracy it was. Bodéüs argues the importance, in Aristotle's eyes, of moral education in the form of law to the duration of regimes and contends, moreover, that Aristotle evaluates regimes on the basis only of their internal consistency. As Paul Vander Waerdt has elsewhere pointed out against Bodéüs, however, Aristotle encourages legislators to work for a second telos, the unqualified betterment of the regime, in addition to preservation.

Lord notes in the Introduction that the collection might have included essays on other topics. A piece on the relevance of Aristotle's biology to his political works, written by Leon Kass or Roger Masters, for example, would, indeed, have been welcome, as would a piece on psychology by, for example, William Fortenbaugh. (Also, Arnhart's book title on p. 44 should be *Aristotle On* Political *Reasoning*.) Not missing or in need of changing, however, is the good index.

Overall, this collection, by improving much predominant thinking about Aristotle's treatment of the human sciences, reveals the Aristotelian challenge to contemporary methods of social science. It also provides a service to the field of political theory by furnishing models of how to illuminate philosophical texts with historical information. In both of these ways, it encourages needed dialogue between social scientists, philosophers, historians, and classicists.

Similar in scope, but not in purpose or unity, to this collection (and providing an *index locorum* instead of a subject–author index), is *A Companion to Aristotle's Politics* (1991), edited by David Keyt and Fred D. Miller, Jr.

Boston University JUDITH A. SWANSON

Ricardian Politics. By Murray Milgate and Shannon C. Stimson. Princeton: Princeton University Press, 1992. 169p. $32.50.

Given Ricardo's reputation as a rather difficult economic theorist and little else, the thesis is novel: "We do wish to claim that Ricardo formulated much of what is characteristic of the democratic political creed of early philosophical radicalism in a distinctive way, that he linked his economic and political speculations in a systematic and novel fashion, and that a re-examination of the grounds upon which he made those arguments is a useful and necessary exercise in historical and theoretical recovery" (p. 6). The incredible thing is that this monograph goes a long way to prove these points.

This is done in a twofold manner: (1) by a meticulous examination of the basis for Ricardo's adherence to a radical reform of the parliamentary franchise (near universality of male suffrage, frequent elections, redistribution of seats, the secret ballot, and greater liberty given to public assemblies) during the period of preparation for, and his actual, parliamentary career (1818–23); and (2) by a continuous counterpoint with the political thought of James Mill, who was, to a certain extent, Ricardo's political mentor. To this is added a subsidiary counterpoint to the thought of Bentham and John Stuart Mill. Were the counterpoint as sound as the reading of Ricardo, this would be a book to place in the same class as Donald Winch's *Adam Smith's Politics* (1978). But there is a major problem in the comparison with the thought of James Mill.

Ricardo is placed in a new light by suggesting that his thought was far more progressive than Mill's in the extent to which he accepted the class-divided nature of the society of his day and was remarkably unafraid of seeing class tensions expressed in the political and parliamentary arena:

> "Ricardo . . . never once thought that this underlying economic rivalry would simply vanish once the vote was granted. In fact, quite the reverse was the case. Even when . . . those competing interests were not just those of competing individuals but were those of different social classes, Ricardo seems to have been perfectly comfortable with the idea that politics existed precisely to allow all those economic interests to gain political expression, rather than to quiet them" (p. 106; see also pp. 118–19).

This is proved beyond the shadow of a doubt by the texts cited from Ricardo's articles, speeches, and letters. For this alone, the work would be worth reading.

But the comparison with the thought of James Mill misses an important problem. It is based upon a close reading of Mill's essay "Government," as well as a certain number of his other works. The difficulty with taking "Government" as representative—or even as the core—of Mill's thought is simple: it is not his *strongest* essay but, rather, one of the *weakest* of the 12 he wrote for the *Encyclopaedia Britannica* between 1816 and 1823, since he wrote it with one arm tied behind his back. Macvey Napier, the editor, had hired Mill to write a series of articles representative of the Benthamite position in social, political, and legal thought (s.v. "Jurisprudence," "Law of Nations," and "Prisons and Prison Discipline," in particular). But Bentham had published his *Plan of Parliamentary Reform* in 1817, and either it or the review James Mackintosh made of it in December 1818 in the *Edinburgh Review* put the fear of God (or rather of political radicalism) in Napier. As a result, Mill was forced to give but a truncated version of the Benthamite position, as the contradictory and feeble nature of much of the argument reveals. Had the authors of *Ricardian Politics* used Mill's later article "The *Edinburgh Review* on Parliamentary Reform" (*Westminster Review*, July 1825), they would have realized that Mill was substantively closer to Ricardo than they here make out. (But their case that Ricardo was more progressive and less backward looking would have still been sustained.) Elsewhere, they also reveal a slightly uncertain hand with regard to Bentham; his unpublished critique of judicial establishments in France (see p. 133) was published as early as 1790, for example; and their remarks about his inadequacies as an economic theorist reveal a certain lack of sympathy with the ends to which his analysis was to be put.

But despite these comments, this book marks a new stage in the consideration of political economy in the development of liberal thought in the nineteenth century. It is lucidly, at times elegantly, written. The authors have an enviable command of the relevant social, economic, and political literature of the period. Their reflections on the links of Ricardo's economic thought to later political radicalism—from the misnamed "Ricardian Socialists" (they were neither Ricardian nor socialists) to the thought of Marx and beyond—are most stimulating. As an essay in revisionism it should go a long way in readdressing how political economy's relationship with political thought is viewed.

University of Toronto ROBERT A. FENN

Managerial Dilemmas: The Political Economy of Hierarchy. By Gary J. Miller. New York: Cambridge University Press, 1992. 254p. $47.95.

Managerial Dilemmas is a fascinating book that introduces political scientists to an array of organizational difficulties with rational underpinnings. Miller is interested in organizational problems that cannot be solved merely by improving communication between superiors and subordinates or by correcting decision makers' flawed thinking. The situations he analyzes share several properties. They can arise even if decision makers have perfectly accurate beliefs about the organization's behavior and outcomes and even think in fully rational ways. Their essential feature is that individually rational behavior can produce collectively undesirable outcomes. Hence, they can justifiably be regarded as enduring dilemmas confronting public and private organizations.

There are, broadly speaking, two types of rational choice scholars in political science. The first type, inspired by models of perfectly competitive markets, believes that institutions are Pareto-optimal. If they were not, someone could make a buck, get elected, or otherwise gain by proposing a new, Pareto-superior institution. The second type, relying on noncooperative game theory, is more pessimistic and is quite willing to believe that situations can endure indefinitely (i.e., as Nash equilibria) yet be collectively suboptimal. Miller belongs squarely in the second camp. In the heart of the book, chapters 4–7, Miller analyzes several important kinds of authority systems and incentive schemes and demonstrates how they can lead to suboptimal outcomes.

The white knight that rides in, in chapter 9, to restore Pareto-optimality to organizations is repeated play. As Miller emphasizes, it is well known that if future payoffs are valued enough, behavior that is not individually rational in the short run (e.g., cooperation in the prisoner's dilemma) can be sustained as a Nash equilibrium in the repeated game. This means that organizationally desirable outcomes can be realized despite the failures of formal authority and incentive systems.

Unfortunately, as Miller also emphasizes, the white knight has a weakness—the folk theorem. This result says, roughly, that when the future is sufficiently important, *any* outcome that gives each player at least that player's "security level" (what he can unilaterally guarantee himself) can be supported as an equilibrium by appropriately chosen strategies. Miller convincingly argues that the folk theorem poses two serious and related problems for decision makers: How will they coordinate on reaching a particular equilibrium? and How will different players fare in the chosen equilibrium? *Managerial Dilemmas* is probably the first book in political science that confronts the implications of the folk theorem head-on. For example, Miller fully recognizes that "repeated play makes cooperation in a team possible, but not inevitable" (p. 214).

Though Miller draws on a technical literature, all the basic ideas are explained in clear English. There is no unnecessary jargon, and no unnecessary mathematical notation or equations. The book is therefore highly accessible to non–technically trained political scientists and organization theorists who are interested in learning about the "new economics of organization." Indeed, it is probably the best introduction to this literature now available to political scientists.

Occasionally the presentation's informality causes problems. For example, chapter 6 examines an interesting impossibility result: given interdependent production that is unobserved by the boss, there exists no incentive system that simultaneously (1) is Pareto-optimal, (2) is a Nash equilibrium, and (3) balances the budget (the incentive system distributes the organization's revenues without deficits or surpluses). Unfortunately, the discussion fails to make explicit that this theorem presumes one-shot interaction. The reader is thus led into believing that the theorem's implications are more far-reaching than they really are.

Similarly, chapter 5 models the difficulty of reaching a Pareto-optimal deal via piece-rate contracts in the context of a one-shot game; again, this context is implicit. Because Miller does not provide an explicit model of piece-rate deals in a repeated game, the reader does not learn whether Pareto-optimality can be sustained as a Nash equilibrium when contracts extend over time. The discussion then moves to an empirical examination of a firm, Lincoln Electric, where such contracts do seem to work. But although the description clearly shows that this case involves repeated play, the reader has not been analytically prepared for the role of ongoing interaction in securing cooperation and may therefore be puzzled why Lincoln's system succeeds.

Miller provides an answer: the owner has *committed* himself to abiding by the terms of the deal (not cutting back on the rate once he sees how much the workers can churn out). But invoking the notion of credible commitment does not clarify matters. Sometimes the idea is used to explain how a particular outcome is selected in a game with multiple equilibria. (One thinks of Schelling's example in which a driver in a game of chicken throws his steering wheel out the window.) But equilibrium selection does not seem to be the whole story in Miller's analysis of the Lincoln case; for in his account, management continues to demonstrate, in an ongoing way, its commitment to the cooperative equilibrium. Thus, credible commitment helps to sustain an equilibrium as well as select one. But because the Lincoln story involves repeated play, where strategies of conditional cooperation can form Nash (and even stronger) equilibria, commitment may be unnecessary. Once the workers and managers of Lincoln are in the cooperative equilibrium, little is added to the analysis by saying that the parties have committed themselves to their strategies. Each person is simply playing optimally, given everyone else's strategy; and this is all that standard Nash equilibrium analysis has to say about the matter. One therefore suspects that Miller is going beyond the standard analysis. In several places, he refers to the "fragility" of the cooperative equilibrium and to the role of managerial commitment in making that outcome more robust. Miller is onto something here; perhaps future work will clarify these issues.

Miller reaches for an admirable integration of rational choice theory and fieldwork based on social-psychological and sociological perspectives. Sometimes the integration comes off beautifully. For example, the analytical discussion of the bad equilibrium in some companies using piece rates is accompanied by a perfectly appropriate recapitulation of William F. Whyte's descriptions of workers and superiors locked in lose–lose conflicts.

Occasionally, the integration is more forced. Miller tries to argue that "mechanistic" hierarchical incentive systems are inefficient and that the "warm fuzzy" approaches from the human relations school of organization theory, involving nonhierarchical work teams and close interpersonal ties, help greatly in reaching Pareto-optimal outcomes. But it is not always clear how all of these components of the human relations school help pick out the socially desirable equilibrium. (The role of some components, such as trust, is clear.) Conversely, it is not always clear why hierarchy must go. If it is repeat play that is doing most of the work in stabilizing benign outcomes, then hierarchical authority structures (and even "mechanistic" incentive schemes) could be maintained; they simply have to include the appropriate stick-and-carrot components. Indeed, one could argue, as Miller does, that "leadership" (a more positive word for hierarchy?) is crucial in solving the equilibrium selection problem.

But these problems reflect the ambitiousness of Miller's efforts: he does not shy away from hard problems on the frontiers of organization theory or game theory. At the same time, he has written a spirited book that can be profitably read by strong undergraduates and entering graduate students. It is a fine achievement.

Stanford University JONATHAN BENDOR

Love and Power: The Role of Religion and Morality in American Politics. By Michael J. Perry. New York: Oxford University Press, 1991. 218p. $29.95.

In the contemporary debate between liberals and communitarians, most participants have come to accept some important concerns of their opponents. Communitarians acknowledge the importance of social pluralism and individual freedom, and liberals realize that the moral development of citizens is vital to the political system. However, liberals have difficulty incorporating into their theories conceptions of moral standards that are nourished by groups with a comprehensive conception of the good life—religious groups, for example. Liberals fear that in teaching people how they ought to live, such groups may fail to engender forms of moral development that are compatible with respect for individual rights. At the same time, the political order may need the moral standards taught by groups with a comprehensive view of the good.

In *Love and Power*, Perry argues that too many Americans wrongly neglect the contributions of religious groups in the justification of, and deliberation about, political questions. Perry, an acknowledged Catholic, believes there is a widespread and mistaken assumption that religious morality should be, or can be, excluded from political discourse in a pluralistic society that is committed to freedom. He insists that liberal thinkers seek neutral politics by excluding discourse premised on conceptions of the good that everyone does not accept. He claims that the removal from discourse of unshared premises is itself not neutral because it unfairly excludes from public consideration the moral positions based on religion. Moreover, this exclusion denies the political order the benefits of religious morality.

In place of an impossible neutral politics, Perry defends an "ecumenical politics," in which "beliefs about human good play a basic role in public deliberations about, and public justifications of, contested political choices" (p. 43). In particular, religion offers "a vision of final and radical reconciliation, a set of beliefs about how one is or can be bound or connected to the world—to the 'other' and to 'nature'—and above all, to Ultimate Reality in a profoundly intimate and ultimately meaningful way" (p. 77). Since every religious vision comprises moral beliefs about how one should live, every such conception of what it means to be fully human implies acceptance of some basic responsibility for the well-being of others. In this sense, all religion is political, not private.

According to Perry, ecumenical politics is communitarian because it strengthens political bonds. For example, religious dialogue can celebrate and support basic American values like our constitutional premises about human rights. Also, Perry emphasizes that dialogue based on different and competing religious and moral traditions can serve an integrative function by transforming sensibilities and by bringing cognitive changes. Of course, many readers may think that Perry gives too little attention to the divisions that religious discourse can bring. He insists that ecumenical politics is fallibilist, because it embraces self-critical rationality, and pluralistic, because it respects divergent ways of life as a source of deepening moral insight. These aspects, plus compassion, will lead to political tolerance, not to authoritarianism; the state will not be used for coercion. Contrary assumptions, he thinks, can be traced to the widespread ignorance about religion of otherwise well-educated persons and to the assumption that all religious persons must make the same extreme political demands that are made by some fundamentalists.

Perry correctly argues that political discourse based on the sharing of religious values does not necessarily lead to attempts to enforce a view of the good on others. However, Perry's ecumenical politics is an ideal form of dialogue that will not always be practiced. We need to understand how the political system can draw upon the views of groups with full theories of the good—views that may support moral standards helpful to society—without imposing a comprehensive conception of the good. Some general guidelines may be given, guidelines that should be considered by those taking Perry's viewpoint. Political authorities must support only moral standards that clearly benefit the entire society, and individuals must retain the freedom to reject these standards in their personal lives. Moreover, government must only support the aspects of morality that are not inextricably bound up with a particular view of the good; and government must not proclaim that some conception of the good is superior to other ways of life. Such guidelines require continued commitment to the distinction that Perry too cavalierly dismisses, namely, the liberal distinction between the right (principles of justice guaranteed by the state) and the good (the way of life chosen by individuals). Adherence to this distinction does not preclude Perry's goal, the enrichment of political discourse as citizens share their deepest understandings of the good life, understandings that may be religious in nature.

University of New Orleans ROBERT B. THIGPEN

Equal Justice. By Eric Rakowski. New York: Oxford University Press, 1991. 385p. $49.95.

Justice requires that equal consideration be given to the welfare of everyone affected by some system for distributing benefits and burdens. The presumption is in favor of equal shares; inequality, not equality, demands justification. This is the starting point for most modern discussions of equality, and *Equal Justice* is no exception. But equality of consideration is, as Eric Rakowski's study demonstrates, often an ambiguous, complex, and controversial criterion for choosing among possible distributive schemes. It is, for example, sufficiently indeterminate to accommodate such rival goals as maximizing the aggregate utility or equalizing for each person the consequences of some distributive scheme. Rejecting both of these schemes, that is, utilitarianism and equality of welfare, Rakowski argues that justice is "equality of fortune." This means that "no one should have less valuable resources and opportunities available to him than anyone else, simply in virtue of some chance occurrence the risk of which he did not choose to incur" (p. 1). Thus, no individual should be worse off or better off than anyone else simply because of good or bad luck (e.g., unequal genetic endowments, certain illnesses, a serendipitous discovery or invention). When inequality is the result of "option luck," like a successful or failed business venture, justice does not require that equality be restored. The general idea is that people's shares should be kept equal "except to the extent that their voluntary actions give rise to inequalities" (p. 75). This is a thesis best revealed in its details. What sort of things count as resources? How is their value to be determined? Do those with expansive tastes need more resources than others? Does one forfeit a claim to compensation or help from others by failing to insure against some foreseeable risk? *Equal Justice* is a long and often demanding book. It is also an uneven one.

If justice means equality of fortune, then it seems that every misfortune that one could not reasonably have taken measures to avoid generates a claim for compensation from society. In effect, every *mis*fortune is an *in*justice. Illness, injury, genetic endowments, physical handicaps, social environment, family, opportunity—where any of these constitute a disadvantage for which one cannot be held responsible, a just society will seek a remedy. Where, alternatively, they constitute an advantage for which one cannot take credit, a just society will insist that the benefits of that good fortune be shared. Rakowski is the first to admit that the ideal of equality of fortune, given the complexity and range of factors that must be considered, would be difficult to honor. Indeed, I would imagine it clearly impossible. Rakowski himself notes (to take one instance) that it is probably impossible to determine which part of some successful business enterprise is attributable to voluntary effort and which part is attributable to natural talents for which one does not deserve reward. Rakowski concedes that the theory of equality of fortune will not always (often) yield criteria for feasible policies. In short, *Equal Justice* is a work in ideal theory pretty much removed from a concern with possible transformations of the world. It is, as much of moral philosophy can be, a suppositional inquiry in which the chief concern is to discover what justice means in cases where actual practice and conventions are bracketed as largely irrelevant. Not surprisingly, therefore, Rakowski is at his best when comparing rival principles and criteria for distributing benefits and burdens. He is less successful at probing the sensibilities that might inform conventional understandings of justice. This is especially evident in the chapter where he defends the nonvoluntary harvesting of cadaver organs and, under certain restrictive circumstances, compulsory organ transplants from live donors. Allowing for a moment that "human tissue is a resource" and that equality of fortune mandates some sort of redistributive donor scheme, it seems equally obvious that cultural meanings and shared understandings must count for a lot here. My complaint is not with how Rakowski balances these considerations but with his failure to see that some balancing is necessary.

It is probably inevitable that part 1 of *Equal Justice* will receive the sort of disproportionate treatment that I have given it. This is unfortunate because part 2, where Rakowski outlines a theory of corrective justice in the context of torts and rules of negligence and strict liability helps to expand the thesis of equality of fortune argued for in part 1 and is, I think, more successful in demonstrating when injury requires compensation. It includes an important chapter criticizing Richard Posner's theory that law should aim at the maximization of social wealth. Part 3, however, which takes up questions about the relevance of numbers to life-saving decisions and the killing of animals to benefit humans, stands alone and should have been published elsewhere.

University of Florida ALFONSO J. DAMICO

Contractarianism and Rational Choice: Essays on David Gauthier's "Morals by Agreement." Edited by Peter Vallentyne. New York: Cambridge University Press, 1991. 339p. $59.50 cloth, $18.95 paper.

Peter Vallentyne has brought together 17 important essays discussing David Gauthier's book, *Morals by Agreement*. A central theme that pervades these articles is summed up best by Peter Danielson, who asks, "Why should agents concerned only to satisfy their own preferences constrain themselves by impartial moral principles?" (p. 292).

Gauthier's social contract emanates from an ideal bargaining position in which each individual as a utility maximizer of one's own preferences comes to realize that each person does individually better by accepting constraints against exploitative conduct, in situations, as Jean Hampton says, where each "has good reason to believe that their partners are inclined to cooperate" (p. 40). In a Pareto-efficient world or a free-market setting, characterized by perfect competition, each would (as Jody Kraus and Jules Coleman say) do as well as possible by acting in purely self-interested ways; and in this case, "it would be irrational for any agent to impose restrictions on the pursuit of self-interest" (p. 255). But when, as in the real world, perfect competition does not take place, this situation creates a Pareto-inefficient environment (p. 256). In this setting, each agrees to constraints each individual believes to be conducive to one's own quest to maximize preferences. Individuals would accept, as the main principle of acquisition, the Lockean proviso, which, in the words of Peter Vallentyne, says that interactions are "subject to the constraint that no one makes him/herself better off by making someone else worse off" (p. 7). The principle of *minimax relative concession* (MRC), which encompasses the Lockean proviso, defines, as Hampton says, the fair terms of cooperation (p. 41). To quote Vallentyne, the MRC indicates that "rational agents would agree to an option for which the highest relative concession is as low as possible" (p. 8).

Gauthier's argument for "constrained maximization" suggests that individuals seek to avoid exploitation, in the hopes that each will be able to maximize one's own preferences. In this vein, both the Lockean proviso and the MRC principle are norms that individuals would voluntarily comply with. Here, the basis for a motivation to support constraints is not Hobbes's Leviathan, which might end up trampling the freedoms and rights that liberal regimes are supposed to secure but an agreement entered into by individuals on a voluntary and noncoercive basis.

But can a motivation to support Gauthier's rules be located solely in self-interest maximization arguments that culminate in a need to avoid exploitation? The book's most important contribution to contemporary scholarship is found in the essays suggesting that contrary to Gauthier, individuals support moral norms not solely from rational choice calculations but from the presence of a moral personality, which suggests rules that individuals find morally valuable for their own sake. Jean Hampton, in her insightful essay, argues that Gauthier needs to recognize that implicit in the commitment to avoid exploitation is the notion of respect for persons and that this idea is a necessary "price" we must pay if we are to avoid making ourselves prey for others (p. 53). In the same vein, David Braybrooke wonders how rights for all and moral progress of any sort can be made unless individuals have "humane feelings" for each other; and so long as Gauthier's theory is ambivalent on this point, his argument "offers only mixed assistance to moral progress" (p. 70). Holly Smith argues that for utility-maximizing individuals, adherence to moral rules means sacrifice and that as a result, individuals would have to approach life with an extreme sense of another important moral trait, perseverance (p. 247).

In the same vein, Geoffrey Sayre-McCord argues in his important essay that many individuals have "sound self-interested" grounds, influenced by the logic of moral argument, for "abandoning egoism in favor of morality" (p. 195). In particular, there are individuals for whom the goals of a moral community are a matter of prime importance over and against self-interest maximization or egoism: "This last group is especially important . . . because almost all human beings belong to it" (p. 195). Peter Danielson, in his fine essay, rejects Gauthier's linking cooperation to the market (and thus to a notion of "exclusive property rights") and says that cooperation, agreement, and bargaining presume "appeals to equality and noncoercion" and thus to "precontractual individual rights [that] do not need [to be] fully developed property rights" (p. 107). Here, it would seem, the ground of community is not market relations and property rights but a Kantian respect for persons. Jan Narveson says in his interesting essay that even if the initial bargaining position incorporates the assumption that individuals are predatory, they will forgo predatory conduct in the future because they will each realize that predation wastes "efforts which could instead be converted into mutual gains" (p. 144). But even in this case, moral progress would require a sufficiency of "humane feelings" so that individuals would be able to believe that they could work together in future encounters. Finally, David Copp's excellent essay demonstrates that individuals, even if they conditionally comply with Gauthier's rules in a hypothetical bargaining position, may find, given a variety of moral and nonmoral motives, that in the actual world, it is not "rational to dispose themselves to comply with these [Gauthier's] principles" (p. 223). Often, peoples' moral preferences are integral to their core identities; and these preferences govern their actual choices, irrespective of the requirements of constrained maximization (p. 224).

Gauthier, in the concluding essay, goes a long way to diffuse the arguments of some of his critics. Gauthier argues that his individual is much more accommodationist than he would at first seem: "In his interactions, others are led, through knowledge or belief about his dispositions, intentions, and plans, to behave in ways that enable him to do better, even though he does not maximize, than he could expect to do were he believed to be disposed to perform only maximizing acts" (p. 328). The accommodating individual is more willing than the interest-maximizing individual to accept the norms of cooperative life as principal goods to be striven for, whether or not they can be derived from a rational choice argument.

While this volume is an excellent contribution to the understanding and critique of the leading ideas in Gauthier's approach to the liberal social contract, the volume would have been made substantially more useful if essays had been included dealing with the kinds of political and social policy that are necessary in order to secure the liberal social contract that Gauthier's theory requires. In effect, what is missing here is a concern for the shape of politics in a Gauthier-oriented regime. The article by Donald Hubin and Mark Lambeth showing that the Lockean proviso is not an adequate foundation for rights is very helpful in this regard; but more essays of this sort dealing with questions like the importance and place of various rights should have been provided (p. 126). Finally, in this regard, how is the rational choice approach useful in confronting many of the current problems facing liberal societies, like abortion, medical care, welfare reform, race, and so on? By not addressing concerns of this sort, the book, while excellent in explaining and providing telling criticisms of Gauthier's concept of constrained maximization, fails to demonstrate the importance of these arguments to the current effort both to justify and sustain the liberal social contract.

Miami University, Ohio STEVEN M. DELUE

AMERICAN POLITICS

The Logic of Congressional Action. By R. Douglas Arnold. New Haven: Yale University Press, 1990. 282p. $29.95.

The purpose of this book is to explain why Congress enacts the policies that it does. Amid the many issues this concern raises for Douglas Arnold, one question is paramount: Why does Congress sometimes break free of particularized interests and enact legislation that serves the general public? Since reelection is the primary concern of legislators, Arnold asserts, incumbent support for particularized group or geographic interests fits their needs by facilitating "advertising" and "credit claiming." But these same legislators sometimes approve policies that deliver collective goods to the general public while imposing demonstrable costs on voters. Why?

To answer this question, Arnold creates the most sophisticated model of Congress yet proposed by electoral incentive theorists. In Arnold's world, legislators are concerned not just with the *expressed* preferences of attentive citizens but with the *potential* preferences of an inattentive public. After all, legislators might respond so completely to particularized interests in a policy area as to produce general policy effects detrimental to the public as a whole and thereby generate electoral backlash against themselves. Legislators must thus develop extraordinarily fine-tuned electoral antennae, constantly estimating not just immediate policy positions of groups but probable public reactions once a program's effects begin to be felt.

Proponents and opponents of policies develop procedural and statutory strategies that cater to legislators' electoral calculations. In particular, they arrange voting processes so that electorally beneficial policy decisions will be traceable to visible public votes, whereas detrimental decisions will be less visible. And they draft legislation so that electorally beneficial policy action will have immediate and salient policy effects, whereas detrimental policy effects will be delayed and less salient. Once these and related issues are understood, Arnold argues, it then becomes evident how and why Congress can enact policies serving general, rather than particularized, interests. It can do so when a program's general beneficial impact is salient, or potentially salient, to a substantial number of citizens; when coalition leaders can employ strategies that allow citizens to connect the positive general effects to their legislators' votes; and when coalition leaders can hide the connection between the legislators' votes and any negative particularized impact of the program. Arnold illustrates these processes with case studies drawn from the 1970s and 1980s, concluding that during these years legislators' electoral calculations supported general, as well as particularistic, policymaking.

Arnold's effort in developing and illustrating this thesis is impressive, producing one of the most illuminating books I have read on Congress. Every page provides a new insight into some aspect of congressional policymaking. Students of congressional elections, coalition building, roll-call voting, and legislative careers; specialists in budgetary and economic policy, tax policy, and energy policy; theorists of political representation and rational choice analysts—all will find nuggets of golden insight into their special concerns in Arnold's analysis. Arnold's differentiation between actual and potential preferences and legislators' concern for the foreseeable effects of policies are major innovations in electoral incentive theory. And the effort to demonstrate that electoral calculations can account for the enactment of general, as well as particularized, benefits is one of the great tasks confronting students of representative democracy. For all the brilliance of the effort, unfortunately, the book fails to deliver on this central goal.

The primary problem with Arnold's argument lies in a confusion over the distinction between particularized benefits and collective goods. The allure of Arnold's basic puzzle flows from the long-standing debate over whether a representative assembly can serve the public interest by providing for collective goods, particularly when private interests and particularized goods would seem to be the salient concerns of voters. At the outset, Arnold identifies his effort with this classic debate, arguing that general benefits refer to "collective goods that people value because they believe that everyone profits," with such benefits falling "uniformly on members of society" (p. 26); particularistic goods, by contrast, are the benefits that fall on identifiable group or geographic interests. The trouble is that Arnold assumes that group or geographic interests are the only particularistic interests that concern voters. In point of fact, much of the tension between private and collective interests arises from citizen's desires for highly salient, specific, and divisible *individual* benefits, illustrated by their concern for policies that maintain low prices and, in the post–Proposition 13 world, lower taxes. Electoral incentive theorists have long considered such individualized and private benefits to be the "meat and potatoes" of reelection campaigns, with incumbents invariably positioning themselves to claim credit for such salient benefits.

By equating particularized policies solely with those that relate to group or geographic benefits, Arnold includes in his general policies the programs that deliver private and divisible goods to individual citizens. Arnold's theoretical logic and empirical proof then becomes preoccupied with demonstrating that Congress is willing to enact policies that deliver highly salient, specific, divisible and individualized benefits. The resulting analysis provides powerful proof that private and particularized *individual* benefits can compete with particularized *group* or *geographic* interests for the support of electorally attentive legislators. It does not demonstrate that the electoral calculations of legislators support the enactment of general policies that deliver diffuse collective goods.

Missing from the book is a serious consideration of what constitutes a collective good. General programs providing collective goods probably should be expected to meet at least three conditions: (1) the program's primary benefit should be substantial, shared, and indivisible among citizens (e.g., clean air); (2) there should exist real reason to believe that the enactment of the policy would deliver the shared and indivisible benefit (so that such claims are not simply symbolic); and (3) the program should have some substantial private costs, so that government action is required to obtain the beneficial effect. The existence of a collective good cannot be assumed simply as a result of the reversal of group or geographic interests, as a result of an assertion by coalition leaders of a collective benefit, or as a result of broadly distributed effects of the policy. Rather, there must be substantial reason to believe that a real, shared, and indivisible benefit would accrue from government action.

Applying this definition, none of Arnold's policy cases could be convincingly demonstrated to be general policies designed to provide collective goods. Most clearly provide individualized and private goods (low prices or limited taxes) or restructure the distribution of particularized benefits among individual, group, and geographic interests (the combined taxing and spending cuts of 1981 and the Tax Reform Act of 1986). Others are largely symbolic programs enacted at a time when no real prospect existed for their implementation (gasoline rationing in 1980) or drafted in such a way as seriously to decrease the likelihood of full implementation (as with Gramm–Rudman). In addition, appeals to the collective benefits of programs were often based on highly suspect scientific theory (as in the appeal to supply-side economics to support Reagan's tax and spending cuts).

Seen in this manner, Arnold's logic and case studies explore not how electoral calculations and the reliance on strategic ploys account for the provision of collective goods during the 1970s and 1980s but, rather, just the opposite. Arnold has chosen to study congressional policymaking in a period in which the great cyclical mood swing of American politics (as demonstrated recently in the statistical analysis of James Stimson) was moving away from a sustained concern with collective goods and toward a concern with private goods and individualized benefits. This move took place even in the face of considerable argument that such developments would create great collective costs. He is also studying Congress in the wake of the reform movement of the early 1970s that decreased the institution's capacity for sustained and independent deliberation, and expanded the capacity of citizens and interests groups to audit legislators' individual policy positions. The reform movement thus made members highly vulnerable to the growing public pressure for salient, specific, divisible, and individualized goods. What Arnold has demonstrated is that coalition leaders exploited legislators' increased vulnerabilities in the 1970s and 1980s in order to pursue an expansion of privatized and individualized benefits and to reverse historic concerns with collective goods and the general public interest.

The pressing theoretical question is whether the logic that Arnold outlines will work when the public again becomes concerned with collective problems. Do the same electoral calculations and coalition strategies that produce support for private goods and individualized benefits also generate support for collective goods? I have my doubts. It is simply much easier to design policies that provide immediate and specific benefits of a private and individualized nature than it is to design collective policies that do so (seen in the contrast between the immediacy of lower taxes or restrained prices and the long-term benefit of, e.g., cleaner air). How, then, does one proceed who truly seeks to enact diffuse and shared collective benefits? Can Arnold yet demonstrate that the electoral calculation of legislators is compatible with the provision of collective goods? I believe that he can; but to do so, he must make at least two additional adjustments in his theory.

First, Arnold must develop a more sophisticated interpretation of the voting public, with citizens, as well as politicians, attentive not just to immediate policy benefits but to long-term policy effects and concerned, therefore, not just with private and particularized policy agendas but with shared and indivisible policy benefits. Such an approach would place considerable emphasis on the role of ideas and political explanation in electoral politics, with the prospect that the greatest electoral weapon to sustain incumbents in office may be policies that demonstrate the collective benefit of government. It would also need to recognize that because voters assess private, as well as collective, effects of policies, government must provide collective benefits without undue private costs, and in a manner that ensures that the private costs that are incurred generate the desired collective benefits.

The incorporation of such considerations into electoral incentive theory would then require a second innovation, a determination of the institutional arrangements that facilitate authentic congressional deliberation over whether to deliver (and how best to fund) collective goods. Arnold alludes to such concerns briefly in the concluding chapter and appears to believe that a realistic path to a more responsible Congress comes through voting procedures that inhibit floor amendments (thereby undermining particularized pressures on members) and through increased visibility of final votes on substantive legislation (thereby stressing the salience of such votes). My suspicion is that such strategies, in and of themselves, will only increase the electoral pressures on legislators to respond to the desires of citizens for salient short-term divisible individual benefits. For Congress to reassert its legitimacy as a policymaking institution capable of pursuing collective goods in a responsible manner, it must strengthen its age-old stalwarts of collegial decision making: the parties, centralized leadership, and a deliberative committee system. The ultimate key to such strengthening is for politicians, journalists, citizens, and academicians(!) to grapple with the central issue of democratic government, namely, whether there are collective goods and a public interest that we require a representative government to address.

These, then, are my reactions to *The Logic of Congressional Action*. Others will undoubtedly respond to Douglas Arnold's arguments and closing reflections with different concerns. It is to Arnold's credit that he has tackled one of the most central and unsolved puzzles of political science and has framed the debate in new, innovative and provocative ways. *The Logic of Congressional Action* is destined to become a classic investigation of congressional decision making and a welcomed stimulus to a new generation of theorizing and policy analysis. Arnold has yet to demonstrate that the electoral calculus of legislators yields general policies providing collective goods, but he is clearly knocking at the door.

University of Colorado, Boulder LAWRENCE C. DODD

Nominating the President. Edited by Emmett H. Buell, Jr. and Lee Sigelman. Knoxville: University of Tennessee Press, 1991. 300p. $42.95 cloth, $18.95 paper.

Super Tuesday: Regional Politics and Presidential Primaries. By Barbara Norrander. Lexington: University of Kentucky Press, 1992, 239p. $29.00.

In introducing *Nominating the President*, Lee Sigelman argues that political scientists often focus on the presidential general election "to the virtual—or, in a few cases, total—exclusion of the most time-consuming, expensive, and formative portion of the campaign" (p. xiv). His book with Emmett H. Buell, Jr., he explains, is an attempt to "rebalance the scales through a sustained examination of the politics of the presidential nomination process" (ibid.).

Although Sigelman exaggerates how much scholars have ignored the nominating process (as the chapter references and the "Selected Bibliography" reveal), the book provides an important contribution to understanding the nominating process in general and the 1988 campaigns in particular. And Barbara Norrander, who is both a contributor to the Sigelman and Buell collection and the author of *Super Tuesday*, provides what will probably be the definitive analysis of the attempt by Southern and border states to influence the nominating process by coordinating their primaries on a single day, 8 March, 1988.

Nominating the President contains nine chapters. Buell and James W. Davis analyze the strategic environment of the 1988 nomination race and provide an especially valuable analysis of the Republican and Democratic delegate selection rules. Alan I. Abramowitz, Ronald B. Rapoport, and Walter J. Stone employ their own mail-back survey of Democratic and Republican participants in the 1988 Iowa caucuses and demonstrate the extent to which the Iowa campaign is based upon personal contacts between candidates and their organizations and the caucus participants. Norrander studies voting behavior in the 1988 Super Tuesday, mainly by analyzing the National Election Study's Super Tuesday survey.

Clyde Wilcox provides a detailed analysis of campaign fund-raising and spending in the 1988 contest but acknowledges that it is difficult to pinpoint the effects of campaign spending without an extensive analysis of state polling data and free media coverage. Kenneth D. Wald discusses the importance of religious appeals by both Pat Robertson and Jesse Jackson, arguing that their campaigns demonstrate the permeability of the political parties, the strength of moralistic appeals, the importance of churches as a base of political action, and the diversity of religious appeals in America. Buell studies press coverage of the candidates through a content analysis of campaign coverage in the *Atlanta Constitution*, the *Boston Globe*, the *Chicago Tribune*, the *Los Angeles Times*, and the *New York Times*. Despite extensive "horse race" coverage, these papers provided readers with substantial information about the policy positions of the candidates. Michael J. Robinson and S. Robert Lichter, on the other hand, study television network coverage, and find that "horse race" coverage prevailed over news about the candidates' issue positions.

Buell and John S. Jackson III provide an interesting history of presidential nominating conventions, arguing that although conventions no longer perform the function of actually nominating presidential candidates, they do perform important secondary functions, mainly by providing an opportunity to unify the party before the general election campaign. Finally, W. Wayne Shannon provides a highly critical appraisal of the current nominating system. Setting forth five criteria for evaluating the nominating process, Shannon concludes that most of them are not met and that "the system is *inherently* subversive of party unity" (p. 264). Although Shannon refers to earlier chapters on media coverage, his harsh assessment of the nomination system does not seem warranted by the eight earlier chapters.

Nominating the President is an important collection that contains a great deal of material interesting to students of candidate strategies, media coverage, and the history of the nomination process. There are two shortcomings, however. First, a book focusing mainly on the 1988 nominating process should have included a chapter on the New Hampshire primaries. New Hampshire was the crucial turning point in the Republican contest. Robert Dole's win in the Iowa caucuses, along with George Bush's third-place finish, clearly led to a bounce for Dole; and he actually gained a lead over Bush in New Hampshire Gallup polls. Bush's skillful effort to turn back Dole was crucial to ending Dole's momentum. Second, the editors could have provided a concluding chapter that tied together the earlier chapters, pointing to lessons about the nominating process. Although Shannon's final chapter does draw to some extent upon the earlier chapters, Buell and Sigelman could have provided a more balanced assessment.

Of all the events of the 1988 nominating contest, Super Tuesday was the most interesting innovation. On 8 March 1988, three weeks after the New Hampshire primaries, both the Democratic and Republican parties held primaries in 16 states. Of the 11 states of the old Confederacy, 10 (all but South Carolina) held primaries, as did the border states of Kentucky, Maryland, Missouri, and Oklahoma. Two New England states, Massachusetts and Rhode Island, also held primaries. Five states held Democratic caucuses (including Texas, which held both a primary and caucus). Washington state held both Democratic and Republican caucuses. Although there had been a modest version of Super Tuesday in 1984, Super Tuesday 1988 selected far more delegates on a single day than any previous day in the history of presidential primaries.

Norrander's *Super Tuesday* provides a comprehensive analysis of these primaries, focusing on the impact of the coordinated efforts to create a regional primary. She explains the reasons that southern Democratic leaders wanted to establish a common primary date relatively early during the delegate selection season and also examines Republican hopes that Super Tuesday would backfire on the Democrats by persuading Southern Democrats to participate in Republican primaries. For the most part, she shows, the expectations of party leaders were not fulfilled. Super Tuesday disappointed Southern Democrats by failing to produce a moderate Democratic nominee who could compete effectively in the South. It had little effect on the type of candidates who ran, primary turnout, or the rate at which candidates secured the nomination. Super Tuesday did not fill Republican hopes of inducing Democrats to participate in Republican primaries. On balance, Super Tuesday "did not change the basic process by which candidates captured the Democratic and Republican nominations" (p. 179). In her analyses, Norrander supports her conclusions by drawing upon a wide variety of data sources, including campaign spending records, aggregate election statistics, and public opinion polls, including the National Election Study's Super Tuesday survey. In addition to providing an excellent analysis of candidate strategies, Norrander makes an important contribution to the study of campaign momentum by arguing that scholars should pay greater attention to the process of candidate attrition.

It is a tribute to Norrander's scholarship that she reaches rather modest conclusions about the impact of Super Tuesday. Authors often tend to exaggerate the importance of their subject matter, but Norrander resists this temptation. If anything, she may somewhat underestimate the impact of Super Tuesday on the outcome of the Democratic contest. She maintains that "Super Tuesday did not decide the Democratic delegate race, but it did establish the three major players for the last half of the primary season" (p. 175). She later concludes that although Super Tuesday helped Bush bring the Republican contest to a rapid close, "in the more open Democratic nomination fight, Super Tuesday made Dukakis's victory more difficult" (p. 188). I would argue that Dukakis' limited successes in Super Tuesday were almost as important for his ultimate victory as Bush's sweep of the Super Tuesday primaries was for Bush's victory. Super Tuesday helped established Dukakis as the only white Democrat with national support; and although he did not eliminate Albert Gore until the New York primary on 19 April, his Super Tuesday wins were crucial in establishing his front-runner status. Questions of interpretation aside, Norrander's book is a valuable contribution to our understanding of the presidential nomination process.

Michigan State University PAUL R. ABRAMSON

The Technology Pork Barrel. By Linda R. Cohen and Roger G. Noll. Washington: Brookings Institution, 1991. 400p. $36.95 cloth, $16.95 paper.

The federal government's support of research and development programs designed to support commercial applications has had episodic visibility, inconsistent funding, uncertain political support, and little agreement about its success. Market failure, national interest, risk sharing, and the need for large amounts of money up front are the primary rationales used for the federal government's involvement in research and development programs that seek to convert a promising technology into a commercially viable form. As Cohen and Noll note, this mixture of politics, economics, and technology has combined in ways that produce both research and development successes and failures. Their analysis is directed at determining the factors that characterize successful programs, and they intend for their recommendations to provide guidance for executive branch and congressional policymakers.

A central weakness in the book is the authors' uncertainty about what standard to apply in evaluating the effectiveness of federally funded commercial research and development. The authors initially and correctly argue that public sector funding of research and development with commercial applications is distinctly different from private sector funding or management of research and development. Unfortunately, a large portion of the authors' analysis rests upon literature-based comparisons of private efforts. Given the authors' own distinctions, a more appropriate research design would have primarily focused on past successful and unsuccessful federal research and development commercial applications programs to derive evaluative criteria that could be used in the analysis of the six case studies.

The analysis suffers when it compares complex federal research and development programs with the theoretical perfection possible when private firms make research and development investments in commercial applications. This perspective is unfortunate, since one of the key features of the real-world private sector is the very high level of failure both for small start-up firms and long-established firms.

The authors show an appropriate and realistic understanding of the strengths and weakness of relying on six case studies. They do not, however, give sufficient attention to what universe their set of six cases represents. The selection criterion used, in part, by the authors appears to be an interest "to minimize overlap with two previous collections of case studies" (p. 77).

Particularly because the authors evaluate the set of six cases as containing three failures, two mixed instances, and one unambiguous success, the reader needs to have confidence about how these cases were selected. The authors' conclusion, however, that the results of their analysis "obviously justify skepticism about the wisdom of government programs that seek to bring new technologies to commercial practice" (p. 365), may be overdrawn. A review of their own data reveals a success rate of 13%–50% for high-risk projects that the private sector had shunned. This suggests that the process works fairly well. Especially in an economic world where 90% of all new businesses fail within five years and only a fraction of large businesses retain their position on the Fortune 500 list for more than a decade, these success rates are significant.

The analysis gets confusing at times when the authors are inconsistent in their categorizations, for example, in one place identifying one success, two mixed cases and three failures (p. 78) and in another passage listing one success, one mixed, and four failures (p. 365).

There is always a tension in a book of case studies about where and when to draw conclusions. Cohen and Noll draw theirs in several places, causing some confusion because the reader is uncertain whether it is a conclusion or a standard against which the case studies are to be evaluated.

A potentially important contribution made is the authors' attempt to determine when information is revealed that determines a research and development project's success and whether the information is provided in a timely fashion to political decision makers. Each of the six case studies gamely addresses this, but clear conclusions are hard to draw.

A second useful contribution developed is the concept of building demonstration projects earlier than needed so that technical or economic failures can be converted into short-term political successes by giving proponents "brick and mortar" and local payrolls to point to as evidence of success.

The authors' idea of creating a commercial research and development agency and congressional committee seems unworkable and likely to become one more level of unneeded bureaucracy.

Cohen and Noll begin their analysis auspiciously by acknowledging the high risk the government under-takes when it funds and manages research and development commercial application projects that are too difficult for the private sector. In many ways the flaw in their analysis is analogous to trying to compare Small Business Administration success rates to those achieved by commercial banks when only those rejected by banks use the Small Business Administration. Cohen and Noll have taken on a difficult task and have been most successful in their analysis when their criteria are drawn only from other governmental research and development programs. Private sector research and development theories and frameworks are useful for background purposes, but not for evaluating federal research and development projects. Given the increased interest in this area, policymakers may find this book useful in identifying the political, economic, and technical complexities inherent in any large-scale federal research and development commercial applications project.

Ohio State University RAYMOND W. LAWTON

The Presidential Odyssey of John Glenn. By Richard F. Fenno, Jr. Washington: Congressional Quarterly, 1990. 302p. $19.95.

When Incumbency Fails: The Senate Career of Mark Andrews. By Richard F. Fenno, Jr. Washington: Congressional Quarterly, 1992. 309p. $19.95 paper.

As political scientists and political observers, we owe a continuously accruing debt to Dick Fenno for his skills as a scholar, careful researcher, incredibly perceptive observer, and keen analyst of the institutions, processes, and people filling the American political scene. These two books are the second and fifth in Fenno's series on U.S. senators. When Fenno began his extensive study of the Senate, he chose to concentrate on a few senators, studying, among other things, the relationship between their work in Washington and their experiences in their home states. In so doing, Fenno undoubtedly sought to produce a companion piece to *Home Style,* his remarkably acute analysis of how members of the House of Representatives relate to their constituents and how those relationships impinge on the work of the institution. But political events interceded.

These events demonstrated that Senate careers are not quite so predictable as House careers. In 1984, John Glenn ran for the presidency. His career and his relationship with the Ohioans who sent him to the Senate took a turn that, while not unusual for a sitting Senator, strayed from the norm. In 1986, Mark Andrews, another object of Fenno's attention, was upset in his bid for reelection and another pattern for study emerged. Finally, in 1988, a third of Fenno's subjects, Dan Quayle, was tapped as the Republican party's vice-presidential nominee. While Fenno's sample of senators was diminishing, he also found himself in the unique position of having a manuscript on a largely unknown vice-presidential nominee all but completed. *The Making of a Senator: Dan Quayle* was quickly produced and recognized as the definitive (and at times sympathetic) study of the rise to prominence and Senate career of a politician often abused and made the butt of jokes by less careful analysts.

One result of these twists and turns of political events was Fenno's decision to write individual accounts of various senatorial careers, rather than synthesizing the material into one larger work. The two books under review constitute an interesting subset of Fenno's work, the "loser's bracket." In these works Fenno attacks a most interesting question: Why do successful politicians sometimes run notably unsuccessful, and even losing, campaigns?

The Presidential Odyssey of John Glenn looks at the 1984 presidential campaign from one narrow perspective, that of one of the losing nominees for the Democratic nomination. Fenno's method is particularly appropriate for this study. Fenno is the consummate observer, the "soaker and poker." But "soaking and poking" is not random observation. Fenno observes individuals, interactions, and events in context. In this case, the individual was a very public figure, a career Marine Corps officer, a daring test pilot, an astronaut who became an American hero, a losing—and then a winning—candidate for the U.S. Senate, and a nine-year veteran of Senate politics and maneuvering. The context was the most open and public event in American politics, a presidential campaign.

Fenno's observations began with our discipline's accumulated wisdom on how presidential campaigns are run. He applied to that what he had learned about John Glenn the individual from a careful study of all aspects of Glenn's career, then observed Glenn in the presidential campaign. His conclusions are that some of the problems Glenn faced were predictable from a study of his earlier campaigns and from statements of his own views of public service; others followed directly from poor campaign decisions. But in the final analysis, the failure of Glenn's presidential odyssey was a failure to understand the difference in the constituency for a series of Democratic presidential caucuses and primaries and the constituency that is comfortable having John Glenn represent it in the U.S. Senate.

Fenno is most persuasive in arguing that John Glenn has a view of public service—and of his own role as an American hero—that precluded him from reaching out to constituencies, from giving those with particular stakes in the nominating contest a reason for voting for him, from becoming a candidate who evoked passionate responses. Fenno's is a sympathetic analysis: John Glenn never quite figured out what was needed to obtain the nomination; he felt that the public should want him and that he would serve, but not that he needed to convince voters to believe in him wholeheartedly. Nationwide contests are different from those in any single state, even a large one that is in many ways a microcosm of the nation. John Glenn had a certain appeal; but neither he nor his campaign managers (largely strangers who were attracted to his candidacy because it looked like a winning one) ever understood how to convert his very appealing qualities into a winning candidacy.

The loss by John Glenn, the successful Ohio vote-getter, can be explained by his failure to adapt to a new political environment. But what of Mark Andrews? Fenno writes of a "campaigning–governing–campaigning" sequence that is typical of all legislators. The puzzle in this case is how a successful political career could suddenly turn for the worse. Andrews had represented North Dakota in the Congress for more than two decades. Four times, after the state lost its second seat in the House following the 1970 census, he won his House seat in statewide elections. In 1980 he won election to the Senate with over 70% of the vote. Yet he lost in 1986.

Should not Senate incumbents in very small states be as safe as House incumbents? Should not this safety have been especially in evidence for someone like Andrews, who, in Fenno's terms (chap. 2), adapted the governing style of a "House member in the Senate"?

But Fenno's skillful observations and careful analysis demonstrate differences. Andrews worried about Byron Dorgan as a possible opponent and adapted his style somewhat. He thought about a national constituency and adapted somewhat again. But mostly, events—Andrews's explanation of a critical farm vote and filing of a controversial malpractice suit against prominent local doctors—changed the relationship between Andrews and his constituents. Fenno observed Andrews talking to small groups of people. The group gathering over coffee in the little town of Lehr was a typical example. And Fenno saw that the dynamic between Andrews and the locals changed, but Andrews never came to understand that. He not only lost touch with constituents, but he failed to recognize that they saw him differently. Thus, he lost.

These two books tell important lessons about the relationship between politicians and their constituents. They lead us to focus on politicians' perceptions of that relationship, of the role self-deception plays in political defeat. That is a lesson all losers who can reflect objectively on their losses know all too well. Dick Fenno clearly respects and likes the subjects of his studies; some may feel he has become too close to them. But the genius of his work, in my view, is his ability to reflect those positive personal evaluations while still maintaining his stance as an objective and critical analyst, noting lessons about the political process on which we can all draw with profit. A careful reading of the corpus of Fenno's work on the Senate, in these books and the other three, reveals a wealth of insights that add significantly to our understanding of the careers of senators and how the interplay of their work in Washington and their relationships with their constituents affect the life of the Senate and the working of the legislative process.

Colby College L. SANDY MAISEL

Learning to Legislate: The Senate Education of Arlen Specter. By Richard F. Fenno Jr. (Washington: Congressional Quarterly, 1991). 192p. $28.95 cloth, $19.95 paper.

Forging Legislation. By Paul C. Light. New York: Norton, 1991. 273p. $22.95 cloth, $8.95 paper.

When all is taken into account, lawmaking—the enactment of public policy—is the bottom line on the congressional balance sheet. Citizens want Congress to make policy; the legislators want to enter their pet proposals in the statutory ledger. These two works offer complementary insights into the lawmaking process. Paul Light focuses on a single bill, the Department of Veterans Affairs Act, while Richard Fenno approaches policymaking from the perspective of the individual legislator, Republican Senator Arlen Specter of Pennsylvania. Both are "participant observation" studies. Light served as a professional staff member of the Senate Governmental Affairs Committee (John Glenn, D–OH, chair); Fenno's study, another installment of his seminal analyses of senatorial behavior, reflects his travels and conversations with Specter and his staff. Not surprisingly, the conclusions of the two studies converge to offer interesting insights about Congress's assets and liabilities as a formulator of public policy.

Light's book, seemingly aimed primarily at undergraduate students, is a more or less conventional case study of "how a bill becomes a law." It provides a thorough (if a bit verbose—undergraduates may find the many details tough going) overview of an 11-step legislative process (pp. 241–46) from agenda-setting to final passage. Such theorizing as Light attempts appears in a short appendix, where he offers a typology of six senatorial "decision-making styles": rational actor, university teacher (pursuing "the imperfect search for truth"), business tycoon (playing the policy entrepreneur), medieval warrior (seeking to exercise "raw political power" in search of "total victory"), small-town neighbor (trading favors and making decisions through "friendly give-and-take"), and garbage collector (using a bill to "carry other issues that would never pass on their own").

The styles vary both across issues and over the various stages of the legislative process. For instance, the garbage collector role was the "dominant style" at the fact-finding and floor-amending steps of the Veterans Affairs Act but was largely irrelevant during consideration of the Presidential Transitions Effectiveness Act (another Governmental Affairs Committee bill on which Light worked). The styles tax different senatorial resources. For example, rational actors must invest substantial time and energy but little political capital, while the small-town neighbor does not need to invest either (to any significant degree) in seeking policy influence. There are equivalent variations with respect to the needs for information and expertise. Academic students of Congress will find the appendix, containing this set of general theoretical notions, thought-provoking but will be disappointed in the paucity of significant details about their measurement and significance.

Fenno starts from his now-familiar "campaigning–adjustment–governing–campaigning" career sequence. He finds that Specter's "reliance on aggressive, independent, individual effort" (p. 74) characterized his initial election, his adjustment to Senate service, and his subsequent lawmaking activities (esp. passage of the 1984 Armed Career Criminal Act). Fenno highlights the tension between member individualism (heightened, in Specter's case, by his prelegislative experience as a prosecutor and his less-than-comfortable relationship with the Pennsylvania Republican party establishment) and the collective (or "communitarian") effort essential to forge coalitions to enact legislation. While, as always, it is prudent to be cautious about generalizations that derive from a single case, Fenno's analysis, especially when read in conjunction with his treatments, in other works, of John Glenn, Pete Domenici (R–NM), Dan Quayle (R–IN), and Mark Andrews (R–ND) underscores his fundamental proposition that extralegislative considerations profoundly influence lawmaking performance in the Senate.

More specifically, some significant themes emerge from the two studies. For one thing, individual member calculations loom large in congressional deliberations. Light points to John Glenn's desire to leave his mark on what was otherwise a symbolic bill elevating the Veterans Administration to cabinet rank. The Ohio Democrat was unwilling to give the veterans' "iron triangle" its way or simply to move ahead a bill the House had enacted. Rather, he was determined to redress what he saw as an inequity: the inability of veterans who were denied benefits to sue for redress. While Glenn hoped that judicial review would force the Veterans Administration to improve an admittedly ineffective claims process, veterans groups feared that recourse to the courts would undercut their cozy connection to the Veterans Administration bureaucracy. In addition, Glenn's flirtation with his party's vice-presidential nomination diverted his attention from his lawmaking efforts.

Fenno makes a similar point. Specter's approach, his self-confident, individualistic orientation to legislating, shaped his efforts to enact his career criminal bill. He sought to solicit support from the Reagan White House, as well as his congressional colleagues mostly through personal lobbying, one-on-one, rather than through the cooperative action needed to assemble a winning legislative coalition. His inclinations (perhaps rooted in his personality) gave form to his legislative strategy as they did to his electoral strategy, impeding, Fenno implies, his capacity to advance his legislation easily. In any case, members' motivations and predilections impinge directly on their policymaking activities.

Second, coalition-building efforts are central to congressional policymaking. As suggested, Specter learned only slowly about the need to pursue collective strategies. It took him six months of personal struggle to get a slim three-member majority on his five-member Judiciary subcommittee. His individualism led to uncertain relationships with the full committee chair, Strom Thurmond (R–SC), and the full committee staff. He eventually realized the need to compromise and he accepted amendments that altered but advanced the bill. He agreed to a logroll with Representative William Hughes (D–NJ), chair of the House Judiciary Subcommittee on Crime, attaching the latter's pet proposal to the Senate crime bill as the price for getting his provisions into the House legislation, through the full committee, and passed on the House floor. His career criminal provisions survived in conference only to go down to defeat when Ronald Reagan vetoed the crime package. Specter persevered, however, and in 1984 saw his bill, tailored to meet congressional and executive complaints, signed into law. The arduous battle, while successful, illustrates the limits on individualistic effort: Specter's "learning was . . . constrained by the stylistic preferences he had brought with him to the Senate" (p. 125).

The Veterans Administration cabinet-status bill teaches a similar lesson. To make a nonincremental impact on veterans affairs, Glenn had to maneuver adroitly among the veterans groups, which opposed linking judicial review of denied benefits to cabinet status; Senator Alan Cranston (D–CA), who favored judicial review but opposed linkage for fear it would sink cabinet status; and Senator Alan Simpson (R–WY), who insisted on linkage. He had to play hardball to overcome the opposition of House Veterans Affairs Chair, G. V. ("Sonny") Montgomery, who, as a part of the Veterans Administration subgovernment, was against judicial review in principle. In conference, Glenn won clear victories on only three of his six "must" provisions. He agreed to drop the court review from the cabinet-status bill; but when Cranston put a "hold" on the conference committee report on that legislation, Montgomery was forced to accept a separate judicial review bill (which he had been able to thwart on four previous occasions) to protect Veterans Administration cabinet status.

A third moral that emerges from these stories is the critical centrality of staff. Fenno refers to the "Specter enterprise, . . . the senator and his closest aides" (p. viii). Specter's staffers played important parts in setting the agenda for crime legislation; in managing hearings intended to promote interest in, and support for, the career criminal act; and in dealing with the House Judiciary Committee and its staff. Glenn's staff contributed in similar ways. In addition, it drafted statements for its principal. Interestingly, it prepared two statements—one for, and one other against, linkage—because the senator's position on the matter was uncertain. (Glenn read the prolinkage speech.) For the conference committee, the staffers did the lion's share of the work. Together with their House counterparts, they forged the agreements that comprised the language of the bill; Glenn and his opposite number, Representative Jack Brooks (D–TX), met and largely ratified staff suggestions. There never was a "full dress . . . meeting" of the conference (p. 219). Given the multiple pursuits of elected legislators, the staff plays a distinct and often unappreciated role with regard to congressional lawmaking.

Policymaking, as these books portray it, is nothing if not a messy business. While more theoretically inclined scholars would prefer hard-and-fast generalizations, they may have to settle for broad tendencies. Members matter; for their concerns—electoral requisites outside Congress and policy preferences—and decision-making styles give form to what they do and how they act. Term limits or other changes that would bring different sorts of legislators to Congress might produce quite different policy outcomes than does the present legislature. Coalitions count; for in a decentralized institution, even on relatively minor issues such as those Light and Fenno treat, allies are essential and compromise is the order of the day. More centralized, more disciplined political parties would surely change the character of both the process and the content of congressional decision making. Staff is significant; for hard-pressed lawmakers need and use all the help they can get. To reduce staff availability, as some contemporary reformers propose, might well alter congressional policymaking capacity significantly. Senators and representatives, in short, act in an extraordinarily complex environment. These works suggest that unless scholars understand the range of forces that play on the legislators, they may miss much of the subtlety and nuance that help explain Congress' lawmaking assets and liabilities.

Indiana University LEROY N. RIESELBACH

Toward a Usable Past: Liberty under State Constitutions. Edited by Paul Finkelman and Stephen E. Gottlieb. Athens: University of Georgia Press, 1991. 448p. $45.00.

Among the most noteworthy legal developments of the past two decades has been the rediscovery of state constitutions by state judges and legal scholars. The appointment of Warren Burger as chief justice, which seemed to signal a conservative shift on the U.S. Supreme Court, encouraged some state courts to attempt to insulate their rights-affirming rulings from Supreme Court review by basing them on state declarations of rights. Most proponents of this "new judicial federalism" quickly recognized, however, that an opportunistic reliance on state constitutions could not succeed. If they wanted to develop an independent body of state civil liberties law, they would have to ground it in the distinctive language, principles, and history of state constitutions.

This has proved a daunting task. In interpreting the U.S. Constitution, one can draw upon a rich historical record, an abundance of legal precedent, and a wealth of scholarly commentary. In contrast, state constitutional interpreters have often found the historical record scanty, the precedents scattered or unhelpful, and the legal commentary virtually nonexistent. This collection of essays, written primarily by historians and legal scholars, originated as an attempt to alleviate these problems.

This is not to say that this volume promotes a particular constitutional agenda. Morton Horwitz cautions his fellow contributors about "roaming through history looking for one's friends" (p. 148); but the warning seems unnecessary. If anything, professional suspicion of "law office history" leads to an excess of caution. *Toward a Usable Past* will disappoint readers seeking definitive interpretations of state constitutional provisions. However, there are other uses of the past.

The narrowest function of constitutional history is to provide a context for understanding important cases. Several essays in this volume serve this function. Mary Dudziak traces the controversy over birth control in Connecticut prior to *Griswold*, arguing that the state's ban on the distribution and use of contraceptives indeed limited their availability. Morgan Kousser chronicles legal developments regarding segregation in Louisiana prior to *Plessy* and in Kansas prior to *Brown*. Charles Ogletree examines the treatment of the Supreme Court's jury discrimination cases in state courts. While these case studies have some interest, they are not theoretically grounded and do not permit generalizations.

A second function of constitutional history is to clarify changes in constitutional interpretation over time. The essays in this volume focus less on substantive changes than on changes in interpretive approaches. Although state judges have, since the mid-nineteenth century, emphasized textualism and "strict construction" in interpreting state constitutions, three essays reveal a wider range of interpretive traditions in the states. Ellen Peters demonstrates that earlier judges looked to the common law to clarify constitutional meaning, and Suzanna Sherry shows how judges employed standards of "natural justice" in protecting rights. H. Jefferson Powell analyzes the different understandings of judicial review among Virginia judges a decade before *Marbury*.

Perhaps the most valuable function of constitutional history, however, is to illuminate how changes in legal culture have affected the substance and interpretation of constitutions. The several essays analyzing state constitutions in terms of legal and political culture are among the strongest in the volume. Donald Lutz, James Henretta, and Peter Onuf examine the influence of republicanism on state constitutions. Their emphasis on political participation and on the inculcation of virtue underscores the distinctiveness of state constitutional politics for almost a century after independence. Lawrence Friedman's excellent essay analyzes how a concern about "the dangerous classes" during the late nineteenth century influenced the interpretation of state criminal justice guarantees and discouraged the development of new protections for the rights of the accused.

The danger with an edited collection, however, is that some important questions will not be directly addressed by any of the authors. That seems to have occurred in this volume. Most of the essays assume without examination that the similarities in state constitutional development are more important than the differences. Although some essays recognize that the constitutional agenda in the states during the nation's first century differed from the federal constitutional agenda, no essay systematically considers whether such differences continue to exist. Finally, no essay considers whether constitutional history can provide a "usable past" for state constitutional interpreters.

Nevertheless, *Toward a Usable Past* is an important book. The first volume devoted to state constitutional history in recent years, it demonstrates both the importance of the enterprise and how much work remains to be done.

Rutgers University, Camden G. Alan Tarr

Gaining Access: Congress and the Farm Lobby, 1919–1981. By John Mark Hansen. Chicago: University of Chicago Press, 1991. 265p. $38.00 cloth, $15.95 paper.

To what extent and under what conditions do interest groups influence congressional policymaking? Probably no other question elicits as much fascination and consternation from the American public and scholars of American politics. John Mark Hansen's first book, *Gaining Access*, develops and illustrates a theory of access to account for the variable influence of farm lobbies on agricultural policymaking in the U.S. Congress from 1919–1981.

In general, "interest group access results from congressional strategies for dealing with electoral uncertainty" (p. 5). Assuming that reelection is the primary goal of lawmakers, Hansen treats interest group access to the policymaking process as a decision that legislators make in order to obtain the information necessary to secure that goal.

The influence of lobbyists is a function of the degree to which legislators grant lobbyists both access and privileged consideration to the policymaking process. Access is granted (1) "when legislators judge that interest groups enjoy competitive advantage over rival informants" and (2) "when they expect the conditions that maintain competitive advantage to recur" (p. 215). Used repeatedly throughout the work, the term, *competitive advantage*, means "an interpretation of the advice that helped legislators most in their past election," while *recurrence* means "a prediction that the advice will help legislators in their future elections" (p. 6). Access is thus something decided by and for political elites (p. 3).

Though interest groups compete with parties and policy entrepreneurs for access to members of Congress, they have a number of advantages in this struggle for congressional consideration: "First, they provide information and propaganda more efficiently than their rivals. Second, they promote the electoral aims of their congressional clients more effectively than their competitors" (p. 103). As to the classic trade-off between parties and interest groups, Hansen concludes that "strengthening political parties will not overcome the biases and inefficiencies of pressure politics," since "parties cannot absorb the social conflicts that otherwise find their outlets in the pressure system" (pp. 226–27).

Hansen's is not however simply a theory of interest group access. Rather, it is a more general theory of how access to congressional policymaking is determined and influence achieved by any individual or group seeking to bring a demand for authoritative action to the attention of the governing regime. This is a key strength to the book, even if its implications, especially for normative democratic theory, are not fully explored by the author.

Gaining Access is not without a few shortcomings. To begin, some scholars may find Hansen's theory difficult (though not impossible) to falsify. Since there are few objective measures of competitive advantage or recurrence, the reader must accept the author's interpretation of how both were interpreted by lawmakers at the relevant time. If lawmakers continued to grant access because they were misinterpreting the competitive advantage of farm lobbies and the recurrence of farm lobby issues, there's no way to know if access resulted from a misinterpretation on the part of lawmakers or from the traditional "iron triangle" explanations of agricultural policy.

The explanatory power of Hansen's theory of access is ultimately dependent upon a theory of macro agenda building to explain why certain issues become and remain salient for government officials and the polity. Patterns of access, observes Hansen, change as (1) issue salience changes, (2) the contentiousness of issues changes, and (3) elections prove informants right or wrong (p. 105). Beyond noting the "circumstances that create competitive advantage" and "guarantee recurrence," such as economic development, industrialization, population growth, and migration (p. 102), a systematic explanation of how the macro political agenda is created and how it shapes the policymaking process is not provided, limiting the theory to explanations based on the micro electoral calculations of incumbent politicians.

The theory's applicability is also limited by its arena of derivation. Many important studies of interest group politics have been based upon the politics of agricultural policymaking. Yet policies and the politics they engendered are not created equal. Notwithstanding Hansen's illustration that the theory is applicable across time and space to other issue arenas (pp. 217–22), agricultural policy may be distinctive as an election issue, providing greater empirical support for the theory than would another policy. Not only may policies determine politics within the governing apparatus of the state (to borrow Theodore Lowi's influential observation), but certain policies, like agriculture, may better solidify the electoral connection that links interest groups to lawmakers than do others.

Finally, Hansen's theory "works" so long as its underlying assumption, that members of Congress are "single-minded seekers of reelection," is the prevailing norm. Challenge this assumption, and the foundations of the theory begin to crumble. Lawmakers not primarily motivated by reelection may grant access to address important exigencies, rather than to "be on the right side of an issue in the next election" (p. 17). Studies increasingly show that reelection may not be the primary motive of national legislators. The level of public-spirited behavior is important for understanding the results of American policymaking, as are values and ideas. These may be more determinative of policy outcomes than the self-interested pursuit of reelection.

Notwithstanding these criticisms, *Gaining Access* should be taken very seriously as an informative and theoretically suggestive contribution to the study of congressional policymaking and the politics of interests. Hansen's theory of access will become a familiar feature in future studies of interest group politics, and also merits attention in studies of political parties, policy entrepreneurship, social movements, and political consultancy. The theory's widespread diffusion will establish the boundaries of its applicability as it continues to expand our knowledge of how political power is distributed in the American regime.

University of California, Irvine MARK P. PETRACCA

Is Anyone Responsible? How Television Frames Political Issues. By Shanto Iyengar. Chicago: University of Chicago Press, 1991. 195p. $19.95.

This book extends Iyengar's earlier work with Donald Kinder, *News That Matters* (1987), which explored the agenda-setting role of television news. The first book refined the work of McCombs, Shaw, and others and broadened it to include framing effects. The concept of framing refers to "subtle alterations in the statement or presentation of judgment and choice problems, and the term 'framing effect' refers to changes in decision outcomes resulting from these alterations" (p. 11).

The research reported in *Is Anyone Responsible?* examines two ways in which television news frames issues: the episodic and the thematic. The episodic frame is the more prevalent one. It ordinarily takes the form of an event-based news report. An example would be the depiction of the terrorism issue in the context of an Irish Republican Army bombing in Northern Ireland. The thematic frame provides a broader perspective; it reports the issue in the context of "collective outcomes, public policy debates, or historical trends" (p. 18). An example would be a news story that discusses the terrorism issue against the backdrop of the historical bitterness between Northern Ireland's Protestants and Catholics.

On the basis of experimental research, Iyengar concludes that episodic framing on television encourages viewers to assign the blame for society's problems to individuals, rather than to social and political institutions, such as Congress or political parties. Thematic framing is concluded to have the opposite effect. Because television news emphasizes episodic framing, says Iyengar, it deflects the blame for problems from government, resulting in a weakening of political accountability—hence the book's title.

Iyengar's experiments were based on paired groups of participants: one group was shown a videotape that included an episodic news report on a particular issue problem and the second group was shown a thematic report on the same problem. The issues of crime, terrorism, poverty, unemployment, racial inequality, and the Iran–Contra affair were included in the experiments. After their exposure to a twenty-minute videotape, which contained seven news stories, including the story that was subject to experimental manipulation, participants completed a post-test questionnaire that included open-ended questions about the causes and treatment of the problem at issue. Responses were categorized as societal (e.g., a response that attributed crime to impoverished social conditions) or individualistic and punitive (e.g., a response that attributed crime to the character deficiencies of criminals). A comparison of the episodic treatment group with the thematic treatment group revealed that the episodic group's responses usually contained more individualistic and punitive attributions and fewer societal attributions.

The experimental work was backed by a content analysis of all ABC, CBS, and NBC television news stories on the issues in question during the period 1981–86. The content analysis was based on the Vanderbilt Archive's *Abstracts*. The content analysis indicated that although most news stories include both episodic and thematic material, the majority of stories were skewed in the episodic direction.

Iyengar's conclusions are unequivocally stated. Early in the book, for example, he writes: "While there can be no denying the influence of stable dispositional characteristics or cultural norms on attributions of responsibility for political issues, there is compelling evidence that contextual or circumstantial factors are equally—if not more—important . . . . Today, the most important of these contextual influences is television" (p. 10). In the book's final paragraph, he lays the blame for the erosion of political accountability in recent years squarely on television's preference for episodic framing: "The portrayal of recurring issues as unrelated events prevents the public from cumulating the evidence toward any logical, ultimate consequence . . . . Television news may well prove to be the opiate of American society, propagating a false sense of national well-being" (p. 143).

Iyengar's book has substantial merit, but his conclusions are less careful and exact than the experimental research on which they are based. This type of research requires a healthy respect for the limitations of the experimental method. It would have been astonishing, for example, if Iyengar had not found that his subjects, after having just seen an episodically or thematically framed news report on a particular issue, reiterated aspects of the report when asked about the causes and treatment of the issue. To reason from the fact that viewers do repeat such material to the conclusion that network television is behind Americans' tendency toward individualistic attributions is a giant step. This manifestation is deeply embedded in the American political culture and has been for more than two centuries.

Moreover, although it is true, as Iyengar says, that television news focuses "on concrete acts and breaking events" (p. 14), this form of reporting did not originate with television. This model of journalism, which dates at least back to the 1830s, is the dominant form of newspaper reporting and has been taught in schools of journalism since the early 1900s. In fact, newspaper journalists depend even more heavily on episodic reports than do television journalists. Compared with the newspapers' inverted pyramids, television emphasizes the interpretive news report due to its need for tightly structured stories. If stories are to be readily understood by a listening audience, they cannot be allowed to trail off as a newspaper story may. Accordingly, as Dennis Lowery's content analysis indicated (*Journalism Quarterly* [1971]), television news stories tend to be built around inferential sentences, often of an unattributed nature.

It is this thematic quality of television more than the episodic orientation it shares with print that was the foundation, nearly two decades ago, for Michael Robinson's "video malaise" thesis (*American Political Science Review* [1976]). Robinson sought to explain the same phenomena that intrigued Iyengar, but he reached the opposite conclusion. Both cannot be right. Curiously, Iyengar's book contains no recognition of Robinson's notion of "video malaise" anywhere in the text, the notes, or the bibliography. Perhaps Iyengar's view is the correct one. If it is, the problem is more severe than he suggests. It goes beyond television news to *all* news; a focus on events and episodes is deeply embedded in the practice of American journalism, print and broadcast media alike.

Despite its shortcomings, this book is recommended reading for media scholars. Iyengar's experimental methods offer a precision of measurement that media effects research seldom attains. I believe, moreover, that Iyengar's notion of framing effects is one of the truly important theoretical concepts to appear in recent years. Iyengar's challenge is to balance his noteworthy methods and concepts with an equally measured accounting of the substance of American politics and journalism.

Syracuse University THOMAS E. PATTERSON

On the Man Question: Gender and Civic Virtue in America. By Mark E. Kann. Philadelphia: Temple University Press, 1991. 364p. $39.95.

Emmanuel Le Roy Ladurie once suggested that historians could be divided into parachutists and truffle hunters. Parachutists take a panoramic view as they descend to earth. Truffle hunters root around for details on the ground. Mark Kann is plainly a parachutist. *On the Man Question* presents a sweeping thesis and surveys an enormous scene. Kann promises to investigate nothing less than "the intellectual ferment of Restoration England, analyze John Locke's articulation and anticipation of it, and trace its evolution in eighteenth-century and nineteenth-century America" (p. 32). He discovers a dark "other" liberal tradition in America born of a "misanthropic fear of individualism," involving "a desperate search for father figures" and culminating in a culture of "engendered civic virtue" (pp. ix, 14, 19).

Kann's thesis relies upon three large, related claims. The first asserts the truth of Louis Hartz's theory about a liberal Lockean consensus in America, once civic republicanism and patriarchalism are grafted onto it. This entails the second claim: Locke's own political theory is not merely liberal but is, in fact, a "synthesis of individualism, civic virtue and gender" (p. 114). The third claim follows from the first two and asserts that Locke's political theory created an ideological culture within the American republic that sought to "restrain men's selfishness, women's treachery, and young men's passions" by creating citizen–fathers, civic mothers and patriot–warriors (p. 191). In sum, "the subtext of American history" is a quasi-republican, liberal patriarchal Lockeanism that involved "a single-minded effort to make individualism conditional and compatible with social order" by controlling middle-aged men and manipulating young men and women through the rhetoric of civic virtue (pp. 32, 217). We are asked to read this "subtext" in colonial Puritanism, Revolutionary militia service, Jacksonian expansionism, Civil War soldiering, Victorian marriages, Theodore Roosevelt, and World War I conscription—to mention only a few.

Since a parachutist's thesis can carry decisive advantages in historical explanation, what matters is whether the apparatus that supports it is sturdy enough to carry it safely to earth. I wish it were so in this case, because Kann intends well. He has turned to gender as a necessary outgrowth of investigations into democracy, radicalism, and the history of ideas. However, I have serious doubts about Kann's three claims. Least convincing is the assumption that Americans consciously "adopted" Locke's political theory. We might expect even a reconstituted version of the Hartz thesis to provide some substantive historical and textual evidence for the notion that Locke's writings were integral to American political consciousness. But Kann offers nothing to ground assertions that "Lockean civil and political rights were etched into the nation's founding" or that Locke "articulated the democratic hopes and liberal fears that eventually wed an American dream to an American demonology" or "authored modern liberal hegemony, and his American heirs extended the copyright" (pp. 217, 300). Some truffle hunting would help. Exactly who were these "American heirs"? Exactly how and when did they etch and wed Locke into their understandings? Exactly how and when did this become a consensus? The evidence Kann needs to support these conclusions is simply absent. Until such evidence is forthcoming, the idea of America as a "Lockean consensus," no matter how amended, is in no better shape than it was when Hartz developed it in the 1950s, especially after three decades of subsequent criticism.

The question of how Locke's theory should be interpreted raises other concerns. By reading Locke as a civic republican as well as a liberal patriarchalist, Kann hopes to appropriate the republican revisionist argument—and at the same time finesse it—by strengthening the Lockean consensus theory. This is an inventive move; but everything depends upon the plausibility of the idea that Locke is, at the same time, a patriarchalist, a civic republican, and a liberal. Unless these categories can mean whatever anyone wants them to mean, the historical, textual, and contextual bases for these judgments are, in fact, extremely weak. For us to believe them, we have to forget the point of Locke's critique of Filmer in particular and of patriarchalism more generally; rewrite Locke's few references to *private* virtues as though they were the civic ones of Machiavelli or Harrington; and transform Locke's doctrine of the individual, natural law, and Calvinist calling into words about "individualism" or "companionate marriages" that only John Stuart Mill could have written. Unfortunately, Kann's tendency to read Locke as contributing to social relations that did not yet exist or as able to "anticipate," "prefigure," and "pave the way" toward nineteenth-century America (p. 174) only furthers the suspicion that much of his textual interpretation lacks sensitivity to context and history. Confidence is not bolstered by such claims as that Locke "anticipated Louis Hartz's early American consensus" (p. 117). This calls to mind Quentin Skinner's warning that interpretation should not be a pack of tricks we play on the dead.

Kann's third, and most promising, claim about the engendering of civic virtue in America thus hangs on some slender threads. This is unfortunate, because the book raises the important point that political theories often function to sustain unequal relations of power and to legitimate specific cultural norms along dimensions of gender. But Kann provides neither a theory of gender nor even an account of how he intends to use gender as a category of analysis. Given all that has been written on gender in feminist theory in recent years (very little of it cited by Kann), it is also somewhat surprising that the complexities introduced by race and ethnicity are absent in his big groupings of women and men, where only men are divided into young and middle-aged. (As for class, there are only occasional references to the middle-class.) Moreover, Kann's particular application of gender to the study of American history focuses almost exclusively upon manly themes: masculinity, fraternity, filial bonds, and the psychohistorical saga of fathers and sons. Women are fit to these themes as wives and mothers. If this captures a certain reading of Locke or of the so-called Lockean consensus, it does not adequately explain gender relations in American history or exhaust the ways in which the female has been culturally and symbolically represented. Thus, the diversity of real historical women in the United States and the range of their actual political activities, as well as the complexity of representations of the feminine and femininity, are in the end simplified and sacrificed to the large claims of this well-intended book.

University of Minnesota MARY G. DIETZ

Beyond Persuasion: Organizational Efficiency and Presidential Power. By Matthew Robert Kerbel. Albany: State University of New York Press, 1991. 214p. $16.95 paper.

This book purports to demonstrate that organizational efficiency plays an important role in presidential power, a wonderfully interesting idea. Using data from media coverage on 4 Carter and 4 Reagan bills and 26 other Johnson, Nixon, and Ford cases, Kerbel correlates news story content (e.g., complaints about inefficiency or compliments concerning persuasiveness) with administration success. For example, on the two Carter successes studied, 86% of the 206 media mentions of Carter's persuasion said that Carter "used" persuasion, while on the two Carter defeats, only 54% of the 226 mentions of persuasion said that Carter "used" it. The Reagan percentages and those for the whole closely resemble these (87% and 53% and 88% and 56%, respectively). From such differences on a number of topics, Kerbel draws substantive conclusions about which elements matter: "Efforts to bargain were particularly intense and effective in advance of instances when these presidents successfully achieved their objectives" (p. 44). The last substantive chapter also assesses a LOGIT model predicting success on the 34 cases using five variables: three news-story-based variables, unemployment, and inflation. The combination of differences and the organizational variable's LOGIT performance led Kerbel to his efficiency conclusion.

While this work promises to realize the oft-heard clamor for more systematic empirical analyses, it falls short. Some problems derive from conceptual difficulties, others, from purely empirical sources. As most critics see it, presidency research has too often depended on secondary sources of information, such as reportorial anecdotes, which too often carry no systematic validity or (worse yet) provide "disinformation." Yet

Kerbel proposes to build an empirical analysis based entirely on reportorial sources. Even the substantive data (as opposed to controls) in the LOGIT derive from news counts.

Kerbel fails to suggest the validity of these data. In 1965, for example, Larry O'Brien gave an interview to the *Public Broadcasting System* and *Congressional Quarterly* about administration liaison. O'Brien's principal point was that administration practices rarely involve anything other than providing substantive information or organizational efforts—certainly not "arm twisting." This is very similar to Kerbel's findings. Banking chairman Wright Patman, however, seeing the interview transcript, circled all those statements and sent it to O'Brien with the personal suggestion that he would probably need to "take this along with you to your next confessional." One might imagine that sources would *want* to present *their* version. Communicating with reporters, after all, serves to shape how others perceive. Of course, as with O'Brien and Wright Patman, reality varies distinctly from what is reported.

Even when informants do not plan to generate a particular perception, the reports have no greater validity. For example, one of Kerbel's findings downplays the effectiveness of presidential threats. Consider the bargaining situation. If a president threatens a member or faction and the target gives in, no one reports that success—certainly not the administration and certainly not the "target" who has given in. On the other hand, if the president threatens and that threat turns out ineffective, the target may want to talk, that is, build a reputation for constituency commitment. Without direct observation of administration bargaining, however, news counts produce data with a strong bias to reveal ineffective, rather than effective, threats.

At the center of Kerbel's problem lies a question of which strategic interaction to analyze. One characterization of bargaining describes it as involving unitary actors. Reporters tend to focuses on bargaining at this institutional level—the president versus Congress; so, per force, does Kerbel. But bargaining with the president actually involves individuals. If the administration convinces members to change *from* their predispositions, then it has exercised influence. If it then loses, administration efforts may still reflect well on its competence and influence, given that other variables effect the aggregate outcome. Assessing influence requires understanding starting points, not just outcomes. A better LOGIT could help; but Kerbel analyzes only 34 outcomes and excludes the standard controls familiar to Edwards, Covington, and Bond–Fleisher models, which describe predispositions, such as core support, partisan seats, agenda size, election cycles.

At the institutional level, more detail about the administration's agenda seems essential. We know the public (Congressional Quarterly) agenda differs from the administration's internal agenda. Analyzing issues mostly from the public agenda misrepresents the essential: How much administration commitment? When administrations face losing situations on nonpriority issues, might their best strategy generate a losing fight *of necessity*? If Nixon lacks the numbers, might he pursue a defeat that distinguishes carefully between him and his opponents? Kerbel's operationalized strategic situation, based on reporters' asking why this policy failed, might not appreciate Nixon's strategy. Southern Democrats not privy to the plan but wanting the policy might easily blame administration ineptitude. More detail of the initial strategic situation would improve things. Kerbel's questions ring true. But his considerable sensitivities would have gone farther if he had focused on analyzing internal administration materials drawn from archives, rather than on using the media.

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TERRY SULLIVAN

Social Norms and Economic Institutions. Edited by Kenneth J. Koford and Jeffrey B. Miller. Ann Arbor: University of Michigan Press, 1991. 258p. $29.95.

The rational choice analysis of social institutions has significantly advanced our understanding of the role of institutions in social life. The relationship between rational self-interested action and the constraints produced by institutional rules has been the source of a variety of static explanations of institutional effects and dynamic explanations of institutional emergence and change. But an important feature of many social institutions is lacking in most accounts employing the rational choice approach—the effects of social norms and ethical values. Here we confront questions about values that are not reducible to self-interest and rules that constrain choice for nonconsequential reasons.

Social Norms and Economic Institutions, a volume of essays edited by Kenneth J. Koford and Jeffrey B. Miller, presents a variety of attempts to incorporate norms and values into a rational choice framework. The main focus of the essays is economic institutions, but the issues they address and the insights they develop are applicable more broadly to other social institutions. The essays are organized into five categories: a general model of economic organization incorporating norms, an analysis of the role of ethical values in economic decision making, experimental results on the role of norms in simple social interactions, theories of the emergence of social norms, and more applied analyses of the effects of social norms in particular economic settings.

Overall, the essays are thoughtful and represent an intelligent attempt to address the interrelationship among values, norms, and rational action. For this reason alone, the book can be recommended as a point of departure for anyone interested in these important questions. As with any emerging field of study, the positive proposals for understanding this interrelationship are preliminary and open to debate. Such a debate will be furthered by the clarity with which these proposals are presented in the various essays. By sketching briefly some of the arguments in individual essays, I can highlight some of the areas in which I would encourage future analysis and debate. I will not focus here on the essays on either experimental results or evolutionary theories of norm selection, because such literature is probably more familiar to the readers of this journal.

In "Habit, Customs, and Norms in Economics," Koford and Miller offer a general analytical approach to economic activity that incorporates social norms. The authors intend this essay to serve as the framework with which to locate and assess the other contributions to the volume. Thus, it allows us to consider some general questions about the main approach advocated by many of the authors. Koford and Miller conceive of social norms as a subset of the more general categories of habitual behavior and social custom. The discussion is somewhat imprecise, since several overlapping distinctions among norms, habits, and behavior are offered; but the main force of the approach that they recommend is a claim that norms constrain behavior either unconsciously through custom or consciously through the effort to avoid sanctions for noncompliance.

As a recommendation of a conception of social norms on which to ground future research, this approach raises two questions. First, while the conception does capture some of the ways in which norms affect choice behavior, it underestimates an effect that is central to most sociological accounts: norms establish roles and appropriate standards of behavior. In this way, norms affect conscious behavior: People consciously follow normative prescriptions because it is the appropriate thing to do, not merely because they fear sanctions for noncompliance. Second, are norms merely self-enforcing rules (or regularities in behavior) or are they also laws (with the accompanying force of external sanctions)? Norms are treated both ways in the literature and Koford and Miller might have clarified their position on this point. This makes a difference for both explanations of effects and emergence.

More importantly, Koford and Miller argue that explanations of the role of norms, habits, and custom will be found in the "efficiency properties" of these rules. They do not make the standard mistake of arguing that all social outcomes will be optimal. They even show how certain persistent norms may produce inefficient behavior when external circumstances change. They do, however, rest their entire discussion on the collectively beneficial effects of these rules. Norms simplify social interactions by (1) minimizing the complexity of decision making (limiting the spheres of conscious activity) and (2) increasing the predictability of social interactions (by establishing expectations about the actions of others).

For an approach that rests primarily on individual decision making, the invocation of collectively beneficial functions as the fundamental source of explanation is problematic. The standard result of the prisoner's dilemma analysis emphasizes the tension between individual self-interest and collective benefits. If we are to rely on collective benefits and functional needs to explain norms, then these explanations must rest on mechanisms that are not grounded in rational self-interest. In this essay, the emphasis on functionalist logic (minus such a mechanism) does not sufficiently further our understanding.

"Amending the Economist's 'Rational Egoist' Model To Include Moral Values and Norms" by William B. Griffith and Robert S. Goldfarb—and the accompanying commentaries—present a wide-ranging discussion of the methodological and conceptual issues involved in deciding how to treat values and norms in a rational choice model of individual decision making. Griffith and Goldfarb argue for the importance of incorporating moral values into a model of rational decision making and assess the alternative ways in which that might be done. Given their conception of the effects of norms in terms of the effects of moral values, it is not surprising that they opt for modeling normative effects as preferences, as opposed to constraints on choice.

The commentaries challenge the authors' recommendations on the grounds that (1) the importance of norms for economic analysis is overemphasized and (2) the recommended method of incorporation of norms is open to question. I think that the commentators are wrong on the first front but potentially on to something on the second. While the Griffith–Goldfarb suggestions are illuminating, they are not yet persuasive. The effects of norms as constraints should be given more serious consideration, especially if the suggestion by Dennis Mueller that the most important consideration is empirical testability is to be taken seriously. Also, the tension between individual and collective preferences and values should be given more prominence in the analysis.

The final two essays in the volume, "Corporate Crimes and Innovative Punishments" by Peter A. French and "Bargaining and Contracts" by Jules Coleman, Steven Maser, and Douglas Heckathorn, are highly stimulating efforts to understand the effects of principles of fairness and ethical behavior on economic activity. Both are normative in emphasis. French's piece presents a normative argument in support of a principle of ethical behavior (the *principle of responsive adjustment*) for corporate managers, while the Coleman, Maser, and Heckathorn paper is a normative analysis of what fair bargaining outcomes would look like and which principles of bargaining and enforcement might further them. Future research should definitely include more analyses of this kind that enhance our understanding of the normative consequences of such norms. In addition, we would benefit from similar analyses that apply aspects of the logic developed in this volume to positive explanations of the role of norms and values in economic affairs.

Washington University, St. Louis JACK KNIGHT

The Framers and Fundamental Rights. Edited by Robert A. Licht. Washington: American Enterprise Institute Press, 1991. 194p. $19.95.

That ours is an age grown drunk on the rhetoric of rights is attested to by everything from ideologically brutal Supreme Court confirmation hearings, to the endless articles spewing from the law reviews, to the shelves of books being pumped out to satisfy the requirements for academic tenure and promotion. Talk of rights bombards the public imagination.

Sadly, there tends to be a great deal of poverty amid the plenty, however. Most often, the rhetoric of rights has far more to do with rhetoric than with rights. The great flights of fancy that all too often pass for scholarship are rarely burdened by the weight of history or facts. The result has been, in Mary Ann Glendon's phrase, the impoverishment of political discourse. Thus, it is a major event when a collection of essays as thoughtful as those contained in *The Framers and Fundamental Rights* appears. Our public discourse over the nature and extent of rights will be enriched by this good volume.

The Framers and Fundamental Rights appears as the most recent offering of the American Enterprise Institute to the bicentenaries of the Constitution and Bill of Rights. This, the eleventh volume in the series, is the first not under the judicious editorship of Robert A. Goldwin. But Robert Licht has certainly lived up to the high standards Goldwin established. This collection is a model of intellectual depth, stylistic clarity, and ideological balance. It will repay reading by students and experts alike.

The contributions include Benjamin Barber's "Constitutional Rights—Democratic Instrument or Democratic Obstacle?," Judith Best's "Fundamental Rights and the Structure of the Government," Robert A. Goldwin's "Congressman Madison Proposes Amendments to the Constitution," Lino Graglia's "Constitution and 'Fundamental Rights'," Thomas L. Pangle's "Republicanism and Rights," Mark Tushnet's "*Federalist* and the Institutions of Fundamental Rights," and Michael P. Zuckert's "Thomas Jefferson on Nature and Natural Rights." Taken as a whole, they examine the topic of fundamental rights from nearly every conceivable angle.

None of the these splendid essays is likely to rest comfortably with everyone; there is something here to arouse and antagonize anyone who picks up the volume. Yet it is precisely the ability of the authors to provoke us, each in a different way, that makes *The Framers and Fundamental Rights* so valuable: it makes us think about these important issues in new ways.

Generally, the essays agree on certain points, albeit for different reasons. The authors share the underlying belief that there is something terribly wrong with how we have come to think about fundamental rights. Our times are plagued by what Barber calls our "rights obsessions" (p. 36), a view of rights that has largely denied the propriety of politics in the best sense of the word. "Never has there been so much chatter," writes Pangle, "about 'community', 'bonding', 'empathy', 'nurture', and 'gentleness'; and never has there been so icy and thorough a disconnectedness between women and men, between generations, between fellow-citizens and workers and neighbors" (p. 119). When it comes to rights, talk is cheap—in both senses of the word.

The second point in which the authors find some sturdy common ground is the belief that we have lost sight of the historical foundations of rights—where they came from, what they are, and how best to secure them. Thus, we are tossed about on an allegedly philosophic sea with neither compass nor anchor. When it comes to rights, we have simply lost our bearings. Most of the things spoken of as "rights" are but pale imitations of what earlier and more thoughtful generations understood as being worthy of that hallowed name. In particular, as Graglia points out in what is surely one of the most provocative essays in the bunch, "fundamental rights" in the modern sense are nothing more than "mythical supports of a fraudulent structure built and maintained by America's intellectual elite on the conviction that government with the consent of the governed is undesirable in America" (p. 101).

Together, these essays demand of us a serious reconsideration of where we are and how we got here. They urge a return to the fundamental texts of our liberal tradition not to find the answers so much as to remember the questions we should be asking about fundamental rights. This we can do, first, by considering anew how rights are dependent upon government (Best) and, second, by recalling how our Bill of Rights came into being (Goldwin).

Among these very good essays, two demand to be singled out as at once the most profound and the most provocative. Pangle's survey of the history of republicanism and how America sought to reconcile its demands with rights will administer a wake-up blow to those who have nodded off with visions of civic republicanism dancing through their constitutional heads. In a similar way, Zuckert's excursion through Jefferson's theory of nature and natural rights will deepen any reader's understanding of Jefferson's great contribution to the American notion of fundamental rights. These two essays alone make a reading of *The Framers and Fundamental Rights* important; combined with the other essays, it becomes necessary.

Harvard Law School GARY L. MCDOWELL

Interpreting Tocqueville's "Democracy in America." Edited by Ken Masugi. Savage, MD: Rowman & Littlefield, 1991. 526p. $68.65 cloth, $24.95 paper.

At first Masugi's collection appears eclectic—useful for the breadth of its contributors' concerns but held together only by their common subject, Tocqueville's *Democracy in America*. Topics range from women's status in Tocqueville's, as well as contemporary, America to Tocqueville's use of Jefferson's writings on the corrupting effects of administrative centralization. The essays are grouped into three loose sections, dealing with Tocqueville's place in the history of political philosophy, his analysis of American institutions, and his views concerning American character.

But an important theme holds this fine collection together—Tocqueville's attempt to preserve human greatness in democratic times. Most of the contributors note Tocqueville's worry that democracy may undermine liberty. Several build upon this analysis, fleshing out Tocqueville's concern that liberty serve the higher goal of virtue.

Bruce James Smith points out that for Tocqueville, the "taste" for liberty is aristocratic and hence unnatural to the democratic majority. James Caesar outlines Tocqueville's analysis of the institutions (principally a decentralized press, common law lawyers and juries, and townships) fostering liberty by inculcating character traits necessary for local cooperation and proper respect for tradition (p. 311).

In mutually reenforcing ways, contributors point out that for Tocqueville, liberty (improperly understood) may destroy itself, along with proper character. Delba Winthrop argues that rights claims on government for the fulfillment of "equal" needs encourage a fixation upon material equality, destroying diversity and virtue (p. 419). Only when we recognize that rights are meant to secure personal and communal self-government can we encourage honorable conduct (p. 424).

Peter Augustine Lawler points out that even the desire for honor can be corrupted by democracy. For Tocqueville, democracy asserts and demands equality. But we cannot accept being reduced to indistinguishable elements in the mass of mankind. Intellectuals reclaim their individuality by "standing outside" the humanity they themselves "analyze" into a soulless mass (p. 103). Nonintellectuals seek distinction by deifying the universe—and themselves as part of that god (p. 118). In either case, we lose sight of our mortality and our need to preserve our dignity by combating materialism.

To retain our true individuality, we must replace some of our faith in reason with faith in God. John Adams Wettergreen points out that for Tocqueville, Christianity fostered democracy by asserting our equality before God. Yet if we are to check democratic materialism and our consequent susceptibility to soft despotism, we require Christianity's focus upon eternity and the soul (p. 213).

Ralph Hancock adds that religion's utility, for Tocqueville, arises from its truth. While democrats revel in the supposed indeterminacy of their character, religion shows that we have a universal, limited, human nature, rather than any right to limitless satisfactions (pp. 364–65). Only by accepting our limits can we retain the natural grace of our souls and can both recognition and grace take the form of virtuous action. William Kristol even suggests that by inculcating proper morals, we may reclaim, in Tocquevillean fashion, our religious faith (p. 492).

Not all contributors praise Tocqueville's concern with the soul. Edward Banfield castigates Tocqueville for asserting that good societies depend upon good citizens, created by "some central authority" (p. 254). Both John Marini and Caesar contradict Banfield, citing Tocqueville's statement that any "human idea of unity is almost always sterile," whereas God's plan for mankind is carried out by our participation in numerous, seemingly conflicting actions (pp. 282–83, 304). But even Banfield's dissent points out the revolution in Tocqueville scholarship represented in this book. Banfield attacks Tocqueville, the "student of self-interest," for failing to follow Mandevillean precepts. Masugi himself provides another measured criticism of Tocqueville: despite his prescience, Tocqueville "concedes too much to history"; his rejection of equality as a founding idea for political societies leaves the statesman with too little to guide him in ruling (p. xvii).

One may disagree with some arguments raised in this volume (e.g., that Tocqueville conceded too much to the preexisting institutions, beliefs, and practices that, after all, largely form human character). One might also note that Tocqueville's insistence upon the unity of our character and our need to follow God's will in order to live a good life places him within the tradition of natural law and not merely (as is often asserted) of natural rights.

Almost all essays in this volume are quite good. And the book's weakest essay by far (that by Robert Bellah) could not have been excluded without drawing criticism. Bellah's popular work has renewed interest in Tocqueville. But his essay adds none of the textual evidence so sorely needed to show that he understands Tocqueville's thought and applies it properly in his own work. Instead, Bellah continues to use Tocqueville's name and a few strategically chosen passages on "individualism" to further his own political agenda. With this exception, however, the questions raised in this book are precisely the kind one would expect from a serious and thoughtful study of an important thinker.

Oglethorpe University BRUCE FROHNEN

The Political Economy of Defense Contracting. By Kenneth R. Mayer. New Haven: Yale University Press, 1991. 232p. $28.50.

Kenneth Mayer's central argument is that the pork barrel theory of defense spending is both inadequate and inaccurate as a description of the defense contracting process. Instead, he finds the process of defense spending and Pentagon contracting to be largely driven by nonpolitical factors, overlayed by a veneer of congressional credit claiming, position taking, and all the other political phenomena that the Washington establishment uses to make points with the public and gain reelection.

He concludes that "one reason pork barrel explanations are so attractive is that they are simple, parsimonious, and persuasive. They are also mostly wrong" (p. 210). But they persist. They persist because it is in the interest of congressional members, interest groups, defense contractors, and the Department of Defense for there always to be the fallback explanation that a decision was made because of politics or the pork barrel.

Mayer's evidence falls into four areas. He first examines defense political action committee (PAC) contributions to members of Congress, finding that defense contractor PACs give disproportionately to the members of the military committees. This money is also strongly concentrated among those who are generally supportive of the industry and favor high defense spending. His question is whether the money influences votes. He finds that defense PACs are different from other PACs in timing their contributions around both the electoral cycle (the norm) and the budget cycle, so that there is an increase in PAC contributions about the time of budget hearings, markups, and conference committee meetings. His conclusion is that PAC money does not buy votes: "PAC money may provide members with some clues as to where their support lies, but it will not as a general rule guarantee their vote" (p. 96).

Mayer's second area of evidence concerns votes in Congress. Are local interests the primary determinant of a member's attitude on weapon systems? He finds the influence of local spending on the particular project to be relatively minor compared with other factors, such as a member's general attitude toward defense spending.

The third sort of evidence is defense contracting, where he finds the contracts themselves to be relatively nonpolitical. However, legislators typically surround both negative and positive decisions with political credit claiming, blaming, and the like, thus making what is actually a relatively nonpolitical process look very political. He contrasts the selection of prime contractors with the much more political processes involved in military base closures and the politics in Congress of writing restrictive language in authorization bills to attempt to keep programs important to one's district going. The latter, (in contrast to the actual selection of contractors) are very political, as is the selection of subcontractors, where the prime contractor endeavors to spread the subcontracting around to as many states and congressional districts as possible.

Finally, he looks at defense contracting and the electoral cycle, finding that contract awards are given out disproportionately in the two months before each November election but that these are normal contract awards whose processing is accelerated, that is, they would have been given out anyway. Again, credit claiming is maximized, a veneer over a relatively nonpolitical selection process.

Can we believe all this? I think Mayer is right on the essentials, but there are some troubling aspects that we ought to think about. First, in accepting the story that there is a political veneer of credit claiming and position taking over a relatively nonpolitical process, Mayer (and the others in the distributive politics area) is accepting a more sophisticated Washington myth over a less sophisticated, more journalistic one. While we have more evidence to support "our" myth, there are areas where the evidence is just as impressionistic, based fundamentally on quotes from those involved in the process and limited case studies, just like the journalistic descriptions of defense contracting that he rejects. I am thinking specifically of the evidence on contractor announcements.

Second, if we have such a strong political veneer over an essentially nonpolitical process, what is the boundary between the two? What keeps them apart? How strong are those institutional boundaries, and what kind of a theory of bureaucracy do we need to maintain the boundary? Since the public administration subdiscipline has all but destroyed the politics/administration dichotomy, of which the more sophisticated Washington theory is beginning to sound uncomfortably like an adaptation, we need a better theory of bureaucracy to deal with this subject.

Political scientists interested in decision making in the Department of Defense, in congressional–bureaucratic interactions, or in distributive politics will find the data and descriptions of the decision-making process and outcomes interesting and very competently done (e.g., some very comprehensible leverage charts in the chapter on congressional voting); but the major findings have been prefaced in earlier work in the distributive politics area. For journalists, this book will be a major eye-opener.

I strongly recommend it. It is a solid piece of work, with interesting data, good use of several different methodologies ranging from case studies to probit analysis, and one of the few comprehensive examinations of the distributive aspects of an entire department's budget–procurement process.

California State University, Los Angeles J. Theodore Anagnoson

Dual City: Restructuring New York. Edited by John H. Mollenkopf and Manuel Castells. New York: Russell Sage Foundation, 1991. 477p. $39.95.

The Global City: New York, London, Tokyo. By Saskia Sassen. Princeton: Princeton University Press, 1991. 397p. $39.50.

The two books reviewed here are important benchmarks in the study of urban politics. They document the changing character of cities and highlight the increasingly global context of their economic activity. In doing so, *The Dual City* focuses primarily on New York but gives some comparative attention to Los Angeles and London. *The Global City* examines New York, London, and Tokyo—the three major centers of finance and specialized producer services in the world economy. Together, the two books paint a complex picture of the process known as restructuring, the process through which advanced technology and capitalism have modified class relations and transformed cities worldwide.

Sassen's major achievement in *The Global City* is to show that even though New York, London, and Tokyo are located in radically different cultural and national contexts, they display similar trends. According to Sassen, they are shaped by being command-and-coordination posts in an otherwise dispersed world economy. As their role in the manufacture and transport of consumer goods has declined, all three cities have experienced an expanding high-income professional and managerial class but a shrinking middle class. Casual labor and a growing informal economy (i.e., one largely outside of formal regulations, union contracts, and guaranteed benefits) have put much of the population on insecure economic ground.

Heightened affluence among a consumption-minded class of managers and professionals side by side with the increased need and vulnerability in the remainder of the population is at the heart of the "dual city" thesis in the collection edited by Mollenkopf and Castells. Indeed, as both books indicate, the ability of the affluent to pay well for what they want drives up costs for the nonaffluent, sometimes pushing the latter toward privation. A cost squeeze in affordable housing encourages homelessness, for example.

Tokyo is a particularly interesting case. Not only are the beginnings of homelessness evident, but Tokyo now has a significant illegal immigrant population, notwithstanding Japan's tradition of being a relatively closed society. For inhabitants of the Third World, the informal economy of a global city is inviting despite the hardships entailed.

What is the source of these trends? *The Global City* highlights structural change, the transition from an industrial to a postindustrial society. *The Dual City* concurs but gives a more variegated account and brings the account closer to everyday experience in chapters such as "Women Clerical Workers" and "Crime and the Social Fabric." Whereas Sassen concentrates on global processes, the collection edited by Mollenkopf and Castells gives more attention to interconnections between different levels of activity in the postindustrial city.

Both books are attentive to social, economic, and political complexity but also identify a dominant pattern—the diminution of a stable and economically secure middle class. The erosion of stable employment is, of course, the principal contributor to a diminishing middle class. It is plain that the mobility of finance capital and the dispersion of economic production have undercut organized labor and weakened resistance to the casualization of labor.

The two books thus illuminate the persistence of poverty even under conditions of overall economic growth. Specifically, they call into question the argument that welfare dependence has spawned an urban underclass. Social assistance has been eroding, along with the decline of labor unions and the weakening of the "social compact" that mitigated industrial conflict. By inference, the major contributor to the "culture" of poverty is the shortage of permanent jobs with adequate income and benefits. Economic conditions can undermine "family values" and other features of the mainstream culture. In short, casual labor is not conducive to family responsibility and other long-term considerations.

As both books make clear, there is no turning back to an earlier era. For example, stimulation of industrial jobs is no guarantee of economic improvement. In *The Dual City*, Edward Soja's chapter on restructuring in Los Angeles shows that many of the new industrial jobs are low-paying; and, as Sassen argues, a significant amount of industrial work now occurs in the informal economy. In their chapter in *The Dual City*, Ian Gordon and Michael Harloe go to the heart of the matter, citing the reduced incentives for individuals to acquire human capital. The economy itself encourages a live-for-the-moment mentality.

Escaping long-term commitments and obligations is not restricted to the lower class; it is now pervasive. The ability of mobile capital to induce flexible, profit-serving arrangements has altered society in important ways. The weakening of the implicit "social compact" stands out as an especially important example. But the two books reviewed here depict something much more complicated than a society unraveling. There is a dynamic to the process of restructuring. Mollenkopf and Castells, particularly, give sustained attention to a business and professional core capable of concerted action and to the political handicaps of a "disorganized periphery" of women, minorities, and lesser-paid service *and* industrial workers (pp. 17, 402, 404).

It is not possible to capture here the full richness of these two books and the many issues they raise. There is, however, a major line of argument that unites the two books and guides their discussion. Conflict and division are characteristic of postindustrialism; and this is most evident in the tension between a sector that provides upward mobility and increasing mobility and a sector characterized by shrinking opportunities and deepening deprivation. These two sectors "are not separate and distinct, but rather deeply intertwined products of the same underlying processes," according to Mollenkopf and Castells (p. 11). How, then, will social tension be managed? By separation, by selective disinvestment and retreat into "defended enclaves" (p. 8)? Or, as Sassen suggests, by an ideology of collective powerlessness and inevitable subordination to the forces of the market (p. 334)?

Is there another alternative? The democratic ideal would have us realize and appreciate our shared fate, would see us attracted to the aim of an inclusive society of widening opportunities, and would encourage us to develop the appropriate understanding and build the necessary coalitions to pursue that aim. *The Dual City* and *The Global City* tell us much about how difficult an undertaking that will be. They also tell us how necessary it will be in order to avoid living in a fragmented and conflict-ridden world of uncertain long-term promise. By posing that choice, these two books provide a valuable foundation for beginning a more positive reconstruction of urban life in the postindustrial world.

University of Maryland CLARENCE N. STONE

Media Messages in American Presidential Elections. By Diana Owen. Westport, CT: Greenwood, 1991. 198p. $42.95.

The Formation of Campaign Agendas: A Comparative Analysis of Party and Media Roles in Recent American and British Elections. By Holli A. Semetko, Jay G. Blumler, Michael Gurevitch, and David H. Weaver. Hillsdale, NJ: Lawrence Erlbaum, 1991. 197p. $39.95.

Both of these books examine elections and the media; but they do so from very different analytical, theoretical, and methodological perspectives. The Semetko, Blumler, Gurevitch, and Weaver book is a comparative analysis of the formation of campaign agendas in the 1984 American presidential election and the 1983 British general election. The authors focus on the relative power of the media, candidates, and political parties in shaping the news. Diana Owen's *Media Messages in American Presidential Elections* examines the differential effects of political advertising, news stories, polls, and debates on voters' perceptions of candidates and the campaign during the 1984 and 1988 American presidential campaigns. The emphasis in the Owen book is placed on voters' needs and motivations for using the media during the campaigns and how different kinds of campaign communications might meet those needs.

The Formation of Campaign Agendas builds on the classic agenda-setting work of Maxwell McCombs and Donald Shaw to examine how news agendas are shaped. Rather than concentrate on the media's power to set the agenda of public opinion, however, the authors argue that the media are only part of an interactive process in which candidates and parties struggle among themselves, as well as against news organizations, to control the terms of political debate presented in the news. As the authors clearly demonstrate, the process is a deeply political one and is affected by systemic, institutional, and particularistic conditions that shift the balance of control between the journalists and the politicians.

Starting with a lucid explanation of the major similarities and differences in the media and electoral systems of the United States and Great Britain, Semetko and her colleagues set forth hypotheses to examine the power of the press and politicians to affect campaign agendas. The hypotheses explore the extent of respect or suspicion for politicians and the political process, journalists' pragmatic versus sacerdotal orientations to political coverage, the degree of professionalization of election campaigns, and variation in media competition. In testing these hypotheses, the authors present results from a meticulous content analysis of campaign agendas in selected television and newspaper sources, presidential candidates' stump speeches, and British parties' daily morning press conferences.

As with much agenda-setting research, the authors observe the similarities and disparities among the agendas for a given period of time. One major criticism of this kind of work is that it reveals very little about the direction of influence: Who or what set whose agenda? Or were both agendas affected by a third, external force? In order to address this potential problem, the authors introduce data from their observation of, and interviews with, journalists and editorial staffs of the British Broadcasting Corporation and National Broadcasting Company. This is an excellent example of the use of complementary research methods. The authors give insights into the activities of the daily news operations, the time pressures, the sometimes conflicting journalistic roles, and the relative lack of policy deliberation about the election coverage.

The Formation of Campaign Agendas is a well-researched and clearly written piece of scholarship that advances knowledge about the transactional relationships between the institutions of the press and politics. There are three points that the authors or others working in this tradition might pursue in further research. First, the observations of television newsrooms could be expanded to other participants in the process, particularly the campaign and party organizations. This would provide a more balanced discussion of the reciprocal influences of the campaigns and news staffs. Second, additional cases should be examined in order to test the hypothesis regarding media competition. The differences observed between the campaign agendas in the United States and Great Britain may be due to the demands of a commercial, versus a public service, broadcasting system, as the authors suggest. However, more than two cases need to be considered to provide conclusive evidence. Finally, to their credit, the authors examine the content of visual communications. This work could usefully be developed to allow for more than the five visuals per television story measured in the current book and to examine the interplay of verbal and visual messages.

Media Messages in American Presidential Elections also examines the 1984 American presidential election, but in comparison to the 1988 election. The book is in the media-effects tradition and asks about the relative influence of different forms of campaign communications on the voters and their electoral decisions. Owen views voters as active and aware consumers of campaign messages. Employing a "uses-and-gratifications" model, the author examines the characteristics of political advertising, television and newspaper news, public opinion polls, and presidential debates in order to assess how they may (or may not) gratify voters' needs.

The first chapter is devoted to explaining the "audience-centered model of mass media use and outcomes" (p. 15) and the survey research utilized to assess audience evaluations of candidates, uses of campaign communications, and perceptions of media effects. Owen argues that there are two main types of effects: subjective (reflecting feelings about how well voters' needs are satisfied by mass communication) and objective (dealing with cognitive and behavioral change). The author focuses on subjective effects by analyzing the self-reports of survey respondents in two (1984) or three (1988) counties in the United States. By relying on the self-reports of subjective phenomena, the author considers only the audience's conscious awareness of motivations for using the four communication types. The data largely confirm findings from past research.

Assuming that the four types of campaign communications have unique characteristics that will meet particular audience needs, the bulk of the book describes and analyzes campaign ads, news, polls, and debates with examples from the 1984 and 1988 elections. In each media chapter, Owen briefly examines the shaping of message types by candidates, their campaign staffs, and mass media organizations. She also explores how candidates and the media use each message type and presents analyses of the survey data to discuss voters' uses and perceptions of messages. The last two chapters examine more directly how voter needs and motives can be satisfied by the four message types.

Owen presents a number of findings from previous research, as well as from the county survey data. This is both a strength and a weakness. It is a strength because Owen supplements the disparate findings of past media effects research with results from her 1984 and 1988 election studies. However, because so much information is presented, it is difficult to trace the central points of Owen's argument. The uses-and-gratifications model the author employs asks us to focus on the role of the active audience and does not seem adequate to explain the differences in types of campaign communication. Since most of the book describes and analyzes types of political communications and the characteristics they exhibit that might satisfy voters' reported motives and needs, it seems that a more appropriate theoretical framework would take into account the motives and goals of the communicators and the nature of the content, as well as the motives of the voters. In a discussion of image formation, Owen hints at an interactive process of "mutual exchange of perceptions" between campaigns and voters (p. 13) but does not develop it. The author is clearly interested in content differences, since she examines advertising, news, polls, and debates. Her ideas would be better served either by elaborating on the connection between the theoretical framework and the data or by using a transactional or constructionist model (e.g., those of Sidney Kraus and Dennis Davis's *Effects of Mass Communication on Political Behavior* [1976] or David Swanson and Dan Nimmo's *New Directions in Political Communication* [1990]).

Both books are well grounded in the literature on media and elections and should be of interest to students of American presidential politics, political communications, or media influence. *Media Messages in American Presidential Elections* adds data from the 1984 and 1988 presidential campaigns to our understanding of media and elections. *The Formation of Campaign Agendas*, with its comparative, multimethod perspective makes a particularly important and original contribution to the existing literature on agenda-setting and electoral politics.

University of Southern California ANN N. CRIGLER

The Rational Public: Fifty Years of Trends in Americans' Policy Preferences. By Benjamin I. Page and Robert Y. Shapiro. Chicago: University of Chicago Press, 1992. 488p. $59.95 cloth, $19.95 paper.

The Reasoning Voter: Communication and Persuasion in Presidential Campaigns. By Samuel L. Popkin. Chicago: University of Chicago Press, 1991. 291p. $19.95.

Public Opinion in America: Moods, Cycles, and Swings. By James A. Stimson. Boulder: Westview, 1991. 156p. $49.95 cloth, $14.95 paper.

Most areas of research display a predictable pattern, characterized by a surge of excitement and energy, followed by solid, normal science, followed yet later by ever-growing calls for new questions and perspectives. The study of public opinion is no exception. Philip Converse's "The Nature of Mass Belief Systems" served as the catalyst for nearly three decades of systematic inquiry into the nature of people's political attitudes and preferences. But during the past few years, political scientists began to express a concern that research had hit a plateau. The plaintive plea, "We need some new ideas and some alternative ways of thinking about public opinion" dominated more than one dinner conversation at professional conventions.

Students of public opinion owe a great debt to the authors of the three books listed above. Published almost simultaneously, these works, individually and collectively, undoubtedly will provide the impetus that researchers have been seeking. In a very real sense, the next generation of graduate students will be building upon a new and exciting foundation.

For those who place faith in the ordinary citizen, the authors have done much more than generate a renewed interest in the research endeavor. All three books resurrect the place of citizens and their preferences within the context of American representative democracy. Not that any of the authors portrays the typical citizen as a philosopher-king, steeped in knowledge about national politics. But democrats will find great satisfaction in the conclusion that public opinion is far from capricious, arbitrary, and ill-founded. Contrary to the prevailing and rather pessimistic assessment (some might say, indictment) that arose from decades of analysis of individual responses to survey items, these works find in public opinion a capacity to guide public policymaking.

The Rational Public represents the culmination of a project that the authors began a decade ago. Based on a massive data set that combines nearly every existing poll or survey question related to national politics, the study reports, in detail and with great care, trends in Americans' policy preferences between 1935 and 1990. Those who teach introductory American politics or public opinion courses will find a gold mine of information here. Page and Shapiro not only present the trends in easily digestable form but discuss the larger economic, political, and technological contexts that help to explain them.

One who graded public opinion on the basis of reading *The Rational Public* would surely conclude that an *A* is in order; for public opinion, Page and Shapiro argue, meets the "three *Rs*" criterion: it is reasonable, responsible, and rational. Not any public opinion, mind you, but *collective* opinion. The latter, immune to problems arising from measurement error or temporary opinion changes by individuals, reacts in an understandable and a predictable fashion to events, broad social and economic changes, and statements of experts and visible public officials. Moreover, public opinion often influences government policy.

Collective public opinion may be rational, but by no means is it foolproof. Page and Shapiro present striking examples of the manipulation of information by officeholders, especially in foreign affairs, which led the general citizenry to prefer policy options that served the politicians', but not the public's, interests. The rationality of representative democracy depends not only on the rationality of collective opinion but also on a supply of honest and complete information provided by those who have direct access to it. Political competition among elites increases the likelihood, but does not guarantee, that such information will be forthcoming.

Like Page and Shapiro, Stimson conceptualizes public opinion as an aggregate phenomenon—and with convincing justification: "If our topic is people, then [the focus on individual psychology] is relatively harmless. . . . But if the topic is politics, then it matters a great deal if aggregate opinions are more orderly and meaningful than individual ones. For it is the aggregate that matters in politics" (p. 2). Presumably, most politicians and journalists would agree.

Whereas Page and Shapiro have written a long book, Stimson has written a short one (an interim report on the way to a larger study of political representation). Whereas the former construe collective opinion in terms of rather specific policy areas (civil rights, civil liberties, wage and price controls), Stimson develops the idea of *policy mood*. What distinguishes mood from specific policy preferences is that the former involves two aggregations: over individuals and over issues. Mood, then, is akin to liberalism–conservatism (Should government do more or less?) but is a function of a whole package of specific policy preferences.

I cannot begin to do justice to the character and quality of Stimson's work. It represents some of the most sophisticated analysis to be found in the field of public opinion. Moreover, the care and thought that go into the conception and measurement of mood will set a new standard of research. Despite all of the technical complexity and sophistication, Stimson retains a commonsense perspective throughout the book. Truly insightful work always becomes "self-evident" upon completion, a criterion that every chapter in *Public Opinion in America* meets.

Mood, Stimson demonstrates, became increasingly liberal during the 1960s and early 1970s, shifted to the Right during the remainder of the decade, and then (surprise!) turned in an undeniably liberal direction just about the time that Ronald Reagan captured the presidency. Just as Page and Shapiro find rationality in the movement of collective policy preferences, Stimson sees a logic in the cycles in mood, a determination that depends on the author's conception of political representation.

To Stimson (in contrast to Page and Shapiro) the linkage between citizens and representatives is not a direct one whereby public opinion and public policy share a one-to-one (even if rough) correspondence. Rather, citizens respond to government performance. If a generally liberal agenda appears to be working, people will, within a "zone of acquiescence," permit politicians to take the lead in developing specific programs. If, however, they begin to perceive problems, citizens will become less tolerant of government action, which will be reflected in a growing conservative mood. Thus the increased conservatism of the mid-to-late-1970s reflects a growing disenchantment with the liberal agenda that dominated the 1960s just as the shift to a liberal mood during the 1980s reflects a dissatisfaction with Ronald Reagan's policies.

One might argue that Page and Shapiro's and Stimson's similar conclusions about the rationality and reasonableness of public opinion depend wholely on aggregation. Analysis at the individual level, such an argument would go, will inevitably lead to a very different conclusion, as it has for nearly three decades.

Not so, contends Popkin. Ironically, Popkin reaches this verdict by turning to the very research that Page, Shapiro, and Stimson likely find unnecessary and superfluous. Especially important are the insights of psychologists Amos Tversky, Daniel Kahneman, and Jerome Bruner, who, through sustained programs of research, have demonstrated that people use heuristics and other informational shortcuts when making decisions. Three cognitive principles loom especially large in Popkin's work: (1) the representative heuristic (inferring what kind of person someone will be by comparison to "typical" people one knows); (2) the recency effect (weighing recent information more heavily than past information); and (3) the Drunkard's Search (simplifying the decision process by focusing on a limited number of criteria). Reliance on these three heuristics, especially in combination, comprises what Popkin calls "low-information or gut-level rationality."

People do not use these rules devoid of context, however. Thus, the framing of alternatives influences the final judgments as much as the informational shortcuts themselves. With respect, specifically, to electoral politics, candidates compete among themselves to frame the campaign to their advantage. Five changes of frame occur repeatedly in presidential primary and general elections: (1) portraying the incumbent as president or as a candidate; (2) discussing the candidates as people or as holders of public records; (3) portraying the candidates as battling to win the nomination or as certain nominees;

(4) emphasizing economic or international issues; and (5) emphasizing unemployment or inflation.

Popkin then goes on to argue that people in fact make reasoned (and, by implication, "good") choices in primary and general elections. The abrupt changes in support for primary candidates make good sense once one recognizes that voters use heuristics and that candidates continually strive, sometimes successfully, sometimes not, to change the prevailing frame of the campaign. Moreover, the mystical campaign momentum that numerous scholars have documented and tried to understand is no more than a result of voters making reasoned choices.

Unlike the other two books reviewed here, *The Reasoning Voter* presents little quantitative analysis. What Popkin has demonstrated, then, is the value of carefully and systematically integrating the literature and then deriving implications.

I inevitably have a few quibbles about each book. Because Page and Shapiro had no control over the timing of the surveys and polls, they cannot completely rule out history effects. Moreover, it may be that the rhetoric surrounding policy proposals does more to influence opinion than the policies themselves. If so, Page and Shapiro may have underestimated how far the actions of public officials hold sway. Stimson unabashedly construes mood in cognitive terms. Perhaps mood reflects a growing anger on the part of citizens toward government. In this light, it would be helpful to know whether the documented changes in mood occur principally among certain segments of the population (e.g., the less interested). At times, Popkin presents as fact what others might consider an open empirical question.

In no way should these quibbles detract from the significance of the works. If there is any doubt, consider two questions political scientists now face. First, if the purpose is to understand the nature and role of public opinion in American representative democracy, should scholars redirect their attention wholeheartedly to aggregate-level, rather than individual-level analysis? That *The Rational Public* and *Public Opinion in America* have just appeared is not completely happenstance. Many disciplines seem headed toward holistic perspectives, as exemplified by the emergence of chaos theory. Ironically, this movement within the discipline is occurring at the very time that political psychology, a highly reductionistic approach, has gained unprecedented prominence. The easy and perhaps correct answer is that we need both perspectives. Whatever one's own choice, the works reviewed here have brought the level-of-analysis problem to the fore, precisely where it belongs.

Second, what criteria should we, as a discipline, bring to bear when evaluating representative democracy? All three works reviewed here reach optimistic conclusions about the quality of public opinion in American politics. Page, Shapiro, and Popkin, like others, view cue taking as an effective means by which people can make proper political judgments. One might argue, however, that the very fact that citizens must look to politicians and other elites for cues underlines a basic problem with large-scale and increasingly complex political systems. "Knowing" via cues may or may not be equivalent to knowing via possessing factual, rooted-in-life information. If not and ordinary citizens cannot gain access to relevant data, what then? Or if, as Stimson argues, mood changes because the citizenry-at-large becomes disenchanted with government programs, why the continuing disenchantment?

Thanks in good part to Benjamin Page, Robert Shapiro, James Stimson, and Samuel Popkin, these kinds of "big" questions beg answers more than ever before. The future of public opinion research could not be more challenging and exciting.

University of Illinois,
Urbana–Champaign JAMES H. KUKLINSKI

The Culture of Spending: Why Congress Lives Beyond Our Means. By James L. Payne. San Francisco: Institute for Contemporary Studies, 1991. 225p. $24.95.

Why do members of Congress vote to increase federal spending in an era of $300-billion deficits? In a lively and contentious study, James Payne argues that members vote for higher spending levels because they are "persuaded" to do so by the pro-spending culture in which they work and by their prior socialization, rather than because of an electoral need to appease voters.

This pro–spending culture is a world filled with pro-spending cues, with few if any that encourage opposition to spending. Rather than serve, for example, as a forum for an evenhanded evaluation of federal programs, committee hearings are "rump hearings," where the witnesses are overwhelmingly in favor of greater spending. Payne's review of witness lists also interestingly reveals that anti-spending groups and organizations, such as the Cato Institute and the Heritage Foundation, rarely make the effort to testify and so to educate members during hearings about why program budgets should be cut. Thus, there is little balance to the testimony presented at these hearings.

While agency officials and pro-spending lobbyists dominate the formal hearings' witness lists, the members' informal contacts and other sources of information, including the media, are also dominated by groups and individuals seeking new programs and increased spending. The "data base" on the effectiveness of federal programs that is available to members, for instance, is heavily weighted toward the proposition that government is the appropriate instrument for solving national problems. This data base is produced by the various agencies and interests that benefit from federal spending, who are loath to recommend anything more than marginal improvements in program performance. The alternative of reducing or terminating federal spending is rarely if ever proposed. Government programs are in fact evaluated by true believers, so that "true critics are excluded from the policy-evaluation process because, simply, they contradict the purposes of those who fund and participate in the evaluation projects" (p. 34). The consequence of all this pro-spending information is not just persuasion, but conversion.

Perhaps more important than their surroundings, Payne indicates that prior experience and socialization of members predisposes them to spend. In the case of both Democrats and Republicans, prior governmental office-holding is positively associated with a greater tendency to vote for spending. Moreover, the longer one has served in Congress, the greater the tendency to spend. When distinguishing between Democrats and Republicans, Payne finds that representatives of the two parties exhibit very different frames of reference when they consider whether to vote for a spending bill. Democrats emphasize what Payne calls a "manifest" impulse, which "leads them to favor immediate government solutions to apparent problems" and thus a tendency to support spending. Republicans emphasize a "latent" impulse and "are more inclined to weigh the long-run costs or harm of a government program, and are therefore less enthusiastic about funding it" (p. 111).

To control spending, Payne advocates creating a culture of anti-spending. This counterculture would include a federal agency that would supply members with information on the inefficiencies and ineffectiveness of federal programs; closed hearings that would shut out lobbyists; a ban on witnesses' testifying before the same committee two years in a row; congressional term limits; restrictions on the amount of time a member might serve on a particular committee; and cost–benefit analysis and program evaluations that would consider the benefits occurring from leaving proposed expenditures in the private sector. Significantly, Payne rejects procedural reforms, such as Gramm–Rudman–Hollings, because members easily find ways to circumvent legal spending limits.

Payne's study is a well-written work that reminds us of the importance of information, attitudinal predispositions, institutional culture, and socialization in budgetary decision making; and he warns that not all spending votes are tied to electoral purposes. The author, furthermore, tells us what any lobbyist working the Hill already knows, namely, that organized opposition to spending is limited and that in their analyses, agencies like the General Accounting Office regularly pull their punches and recommend only marginal programmatic changes.

The culture Payne describes, however, is more complex than he allows. Particularly during the last 12 years, for example, members have regularly heard testimony, regardless of the truth of the matter, that deficit spending would cripple the nation's economy. This theme is constantly replayed by the media; and, to name only one official source, the Congressional Budget Office annually identifies potential program and budget cuts. Moreover, although this study focuses on Congress, the Reagan–Bush administrations have regularly refrained from vetoing appropriations bills, thus significantly contributing to the culture of spending. Finally, although Payne offers welcome relief from the mechanical decisions offered by public choice voting theories, he underestimates the importance of the voters' influence on congressional spending decisions. Members are not paranoid when they think twice about making deep cuts in middle-class entitlements like Social Security. This point is especially important, because the big spending growth in the budget resides in the entitlement, rather than the discretionary accounts.

These comments aside, Payne's book is a valuable addition to the budgetary literature, one that renews the importance of the sociological–cultural framework for studying spending decisions. This book would be appropriate for both undergraduate and graduate students.

University of Virginia James D. Savage

Toxic Politics: Responding to Chemical Disasters. By Michael R. Reich. Ithaca: Cornell University Press, 1991. 316p. $45.00 cloth, $15.95 paper.

The theme of this book centers on the question of how the victims of chemical disasters obtain redress. Specifically, the book takes a comparative view of the politics of chemical disasters by focusing on three such disasters—PBB contamination in cattle in Michigan; dioxin contamination from a factory in Seveso, Italy; and PCB contamination of cooking oil in western Japan. Understanding why the victims of toxic contamination confront similar political obstacles is a major objective of his study. Reich's analysis proceeds in three parts. First, he observes concrete decisions made by political elites and the use of material resources in political bargaining. Next, he pays attention to the processes of agenda formation and analyzes why some issues are organized into politics while others are not. Finally, he analyzes efforts to influence, shape, and determine the wants and demands of powerless groups.

In carrying out his analysis, Reich utilizes case studies as "plausibility probes" for his conceptual schema. The chapters on the three disasters are based on field research conducted during 1978–79, with subsequent follow-up. His methods include collection of primary and secondary documents, as well as elite interviews with approximately 80 major participants in each incident. These participants include victims, politicians, journalists, civil servants, businessmen, private and public interest activists, and legal and medical specialists.

Essentially, Reich argues that the victims of toxic contamination seek redress through politics in three phases: making the problem public, organizing group actions, and mobilizing political allies. In doing so, they often find themselves involved in a process that can "become as poisonous as the chemicals themselves"—"toxic politics" (p. 2). Not long into the process of redress, contamination victims quickly discover that they are powerless. The book concludes with a somber message that incremental tinkering with the regulatory regime may be achievable yet have little impact. Significant change, on the other hand, will have to confront deep political obstacles. Ironically, to obtain even incremental changes in laws, institutions, and goals, more disasters may be required. Thus, to prevent more chemical disasters, "paradoxically and tragically, we may need more chemical disasters" (p. 280).

Other similar books have used the case study approach to analyze the politics of chemical or nuclear disasters. Such books include those on Love Canal (Levine), Bhopal (Shrivastava), Seveso (Conti and Whiteside), and Three Mile Island (Goldsteen and Schorr). In combination with this recent literature, the book suggests that disasters have both detrimental and advantageous effects. From a more positive standpoint, such disasters awaken the public from their complacence and instill in them the understanding that they must take much (perhaps most) of the responsibility for managing their own safety and health, since all too often, corporate and public actors resist their pleas for redress and assurances from technological risk. For example, in Goldsteen and Schorr's *Demanding Democracy after Three Mile Island* (1991), citizens became more favorable toward participatory democracy after discovering that responsible officials misled them in the aftermath of the disaster. In the United States, we have observed the rise of a grassroots movement on toxic chemicals in the aftermath of disasters at Love Canal, Times Beach, and others. The evidence discussed in this book continues to suggest that disasters are bringing about a transformation in the way citizens relate to government. In one instance after another, citizens are "demanding democracy" or even turning toward new forms of citizen participation that may bring them into the forefront of policymaking and implementation.

In summary, this book is a welcome addition to the growing literature on disaster management. More specifically, it could be useful in courses on environmental politics and policy, as well as public policy more generally. While it is constrained by the usual limitations of case study research, it nevertheless contributes a great deal to ongoing discussions about citizen participation in the wake of chemical disasters and contemporary politics of disaster management.

Colorado State University James P. Lester

The End of Realignment? Interpreting American Electoral Eras. Edited by Byron E. Shafer. Madison: University of Wisconsin Press, 1991. 187p. $35.00 cloth, $12.95 paper.

This collection is an outgrowth of the Harold G. Lasswell Symposium at the 1989 meeting of the American Political Science Association. The first three chapters, by Joel H. Silbey, Everett Carll Ladd, and Byron E. Shafer, are revisions of the papers presented at the session. The chapters by Samuel T. McSeveney and Walter Dean Burnham are revised and expanded versions of responses to the papers. An excellent bibliography of the literature on realignment by Harold F. Bass, Jr., completes the volume.

It cannot be reported that this collection will end the continuing debate over the utility of realignment theory (synthesis, perspective, model) either by demolishing it once and for all or by providing new and convincing support. The authors of the first three essays question the capacity of realignment formulations to explain either historical or contemporary American politics. They differ, however, in both the nature and degree of their skepticism. Silbey attributes value to realignment formulations for understanding past, but not contemporary, politics and attempts to subsume such formulations under a broader conceptual framework. Ladd's view is summarized by his essay subtitle, namely, "On the Uselessness of 'Realignment' for Understanding Change in Contemporary American Politics"; and he clearly holds the same view of its applicability to historical politics. Shafer is carefully noncommittal in discussing the utility of realignment formulations for understanding past politics but views them as of little or no value for understanding the contemporary era. All three argue that the political system has passed beyond realignment and no longer has the capacity to realign.

The commentators take different positions. For McSeveney, realignment formulations retain value for understanding political history, although he is more dubious where the present era is concerned. Burnham reasserts the value of the realignment framework for understanding both past and contemporary politics and argues that realignment in fact did occur during the years from 1968 to 1972. He does so, however, by introducing an additional type of realignment, one of non- or even antipartisan form.

Discussions of realignment have rarely been entirely consistent from one to the other and often fall short as models of conceptual clarity or definitional agreement. Unsurprisingly, this collection is no exception. In the initial essays, it is often unclear whether discussions of realignment refer only to the rare historical elections that some scholars have described as realigning or to broader formulations that attempt, whether successfully or unsuccessfully, to describe not only realigning elections but also the politics of the years between realignments.

Discussions of the late 1960s and the early 1970s have similar characteristics. These years are seen as marked by profound political change leading to a new electoral era quite different from those of the past. These changes cannot be seen, however, as realignment—at least not of the "classical" form. The first reaction of a naive reader might be that this may be at least in part a matter of definition. Since the authors make much of the differences between historical realignments, it is not completely clear what the properties of a "classic" realignment are supposed to be. Treating these years as a further example of realignment, only of a different form (as Burnham does), may only add an additional complexity to an already overly complex formulation. The test is, of course, further exploration; but even without such an addition, realignment remains an important (though perhaps time-bound) component of our understanding of historical American politics.

These essays are important contributions to the continuing debate over realignment and to our understanding of American politics, past and present. The panelists at the Lasswell Symposium may have come, as McSeveney suggests, "to bury realignment" (p. 95). Taken in total, however, the resulting volume suggests that burial would be, to say the least, premature.

University of Michigan Jerome M. Clubb

Reasoning and Choice: Explorations in Political Psychology. By Paul M. Sniderman, Richard A. Brody, and Philip E. Tetlock. New York: Cambridge University Press, 1991. 304p. $47.95.

Although woefully uninformed about politics, most citizens manage to provide answers to the myriad demanding and often obscure political questions that confront them in typical mass opinion surveys. How do they do it? This is the central question of *Reasoning and Choice*, a collection of separately written papers woven together by the book's three principal authors.

One possible answer is that bewildered by it all, most citizens respond to surveys in a random or otherwise meaningless fashion. But Sniderman, Brody, and Tetlock, dubbing this the minimalist thesis, reject it as inapplicable to the majority of cases that concern analysts of public opinion, thus setting the stage for the main body of their analysis.

That analysis focuses on "heuristics," or judgmental shortcuts to opinion. The most important of these is the "likability heuristic"—the tendency of people to favor good things, like government aid and freedom of speech, for individuals and groups they like and to oppose good things for those they dislike. Thus, in a series of roughly parallel chapters, the book shows that affect toward blacks, homosexuals, liberals, and conservatives (as measured on hundred-point feeling thermometers) is a reliable determinant of people's opinions on related policy questions. Citizens appear to use the likability heuristic not only for gauging their own policy preferences but for making inferences about the preferences of other groups. They attribute positions similar to their own to liked groups and positions dissimilar to their own to disliked groups. The book further shows that citizens differ in their reliance on the likability heuristic. Well-educated people attach some weight to feelings in making up their minds about issues, but more to ideology; less-educated citizens rely more exclusively on feelings.

Political scientists who do not specialize in public opinion may be unimpressed by such findings. What, they will wonder, have we really learned when we find out that people favor good things for groups they like and bad things for groups they dislike? This is a fair question; but the findings, nonetheless, constitute progress. There is still doubt in the public opinion field whether there is any structure at all in the attitudes of many less-educated respondents. Sniderman, Brody, and Tetlock make a persuasive case that there is, and that it is based on gut-level likes and dislikes.

The book also devotes substantial attention to the nature and operation of political ideology. One finding is that conservatives are distinctly more negative in their feelings toward liberals than are liberals in their feelings toward conservatives. I could not help wondering whether this dour dislike of political opponents might explain why, as another chapter of *Reasoning and Choice* shows, conservatives are less willing than liberals to extend civil liberties to various outgroups, even right-wing outgroups.

Another intriguing finding was that right-leaning citizens appear, in several quite different situations, more susceptible to manipulations of the survey instrument. In one manipulation, respondents were asked whether they supported a certain policy; but half were told that Congress had passed a law in favor of it and half were not. Right-leaning citizens (here measured by low scores on a measure of affect toward blacks) were more apt to favor the policy after being told that Congress had supported the policy (a program to help minorities), whereas racial liberals were unaffected by the experimental manipulation. In another case, self-identified conservatives were more apt than liberals to change their initially stated opinion when the interviewer attempted to counterargue it ("Would you still feel that way even if . . . ?). By way of partial explanation for these findings, the book maintains, with some empirical support, that conservatives may be more conformist than liberals.

A notable strength of the book is its innovative use of Computer Assisted Telephone Interviewing to test substantive hypotheses concerning the motives of different ideological groups. In one experiment, for example, the authors seek to find out whether conservatives or liberals are more likely to take account of the race of a laid-off worker in deciding whether the worker is entitled to unemployment benefits. (The answer is that conservatives are more likely to pay attention to race. They favor a *higher* level of benefits when the laid-off worker is black, but only when told, in another experimental variation, that the worker had been dependable on the job; otherwise, conservatives are, like liberals, unaffected by the race of the worker.)

Because the book covers a great deal of ground, it is open to challenge from many directions. But I found *Reasoning and Choice* an original and stimulating book that merits careful reading and reflection by specialists in the field of political behavior.

University of California, Los Angeles JOHN ZALLER

Politics, Markets, and Congressional Policy Choices. By Peter M. VanDoren. Ann Arbor: Michigan University Press, 1991. 196p. $32.50.

Government intervention in the economy is ubiquitous. The nature, scope, and scale of this intervention suggest strong normative misgivings on the part of politicians about the allocative and distributive consequences of a market economy. The prophylactic policies of Congress engendered by these misgivings are the targets of Peter VanDoren's work.

VanDoren starts with the charitable hypothesis that Congress is well meaning in its micromanagement of the economy and is pursuing in these policies the legitimate provision of public goods or other remedies to classic market failures. Rejection of this hypothesis follows from even the shallowest examination of congressional policymaking. VanDoren then offers alternative hypotheses that range from well intentioned but misapplied policy to the conscious pursuit of goals unrelated to allocative efficiency or, for that matter, distributive equity.

The normative issues related to market intervention are given considerable attention by VanDoren. Intertwined in this discussion are standard microeconomic treatments of relevant topics such as market failure, property rights, and allocative efficiency. For a number of reasons, this is a very useful enterprise. As a service to those less well versed in microeconomic theory, VanDoren defines and clarifies apparent congressional confusion among allocative efficiency, distributive equity, and naked pork barrel politics.

For the central purpose of his book, an empirical analysis of congressional intervention in post-war energy policy, VanDoren combines his discussion of normative issues with a positive theory of congressional policymaking to generate testable hypotheses relating to the formation of energy (coal and nuclear) policy. The positive theory of policy choice seeks to relate the policy preferences of relevant political actors to economic conditions, interest group preferences, and the distribution of costs and benefits. This presentation is somewhat less compelling than the earlier discussion of normative issues. The sampling from the relevant literatures is a bit too selective and proves difficult to synthesize.

The backdrop to the empirical analysis is a two chapter narrative of the history of congressional micromanagement of U.S. energy policy since World War II. This history is indeed a dismal one and VanDoren spares no opportunity to bring to light the consequences of misguided congressional policymaking. In characterizing energy policy as an endless stream of mistakes the author does not hide his own strong beliefs about what goals policy ought to be achieving: allocative efficiency, with distributional issues dealt with by the redistributional opportunities provided for by the economic gains achieved through greater allocative efficiency. Less clear is where the author lays blame. Although Congress is clearly the guilty party, it is not clear whether the mistakes are by design (different policy objectives), the result of congressional confusion about the true nature or incidence of the effects of policy choices, or the result of severe constraints on congressional action such as interest group, presidential, and bureaucratic pressure.

The empirical analysis of energy policy choice is meant to shed some light on this issue. The analysis is fundamentally interested in explaining policy choice: which policy proposals pass, which do not. The empirical model predicting policy choice is fairly elaborate. A two-stage probit model is used to accommodate the exogenous effects of economic conditions, interest group preferences, and the distribution of costs and benefits as well as the endogenous preferences of relevant political actors (members of Congress, the president, and bureaucrats). The unit of analysis is the wording of passages in proposed legislation having discernible microeconomic consequences for energy producers and consumers.

Where VanDoren shines is in recognizing why the results of his analysis might prove valuable. He is not interested in showing that policy is misguided (that is almost taken as a given) but wants to know if reform is politically feasible. To demonstrate this, he recognizes that he must show that reform is in the interest of members of Congress and that they have the ability (discretion) to implement it.

The answer is not self-evident. The empirical model is complex (relative to other policy choice models) and the results show a myriad of pressures bearing on the policy choices of Congress and that these pressures are sensitive to the issue at hand. VanDoren is quite (unusually) candid about what can and cannot be learned from his results. The one obvious casualty is the ability to generalize directly from this study. The somewhat wide-ranging specification search that precedes the estimation and the sensitivity of the results to particular policies tell a cautionary tale.

The caution called for in interpreting the results is no reason to abandon the approach or, for that matter, the model itself. VanDoren's empirical analysis is preceded by an oft repeated and accurate string of criticisms directed at standard policy studies. At a minimum he provides us with a set of methodological tools for moving well beyond standard (pat) policy models.

Some readers are bound to be upset by the case VanDoren makes for the pursuit of allocative efficiency. The goal of moving from the majority rule principle that governs current policy decision making to something approaching the unanimity of market decisions hinges on the successful implementation of a mechanism for redistributing efficiency gains. The logic is there but the feasibility has yet to be demonstrated. It would be a shame, however, if that proved to be sufficient deterrent to try.

University of Texas at Austin BRIAN ROBERTS

COMPARATIVE POLITICS

Explaining Economic Policy Failure: Japan in the 1969–71 International Monetary Crisis. By Robert C. Angel. New York: Columbia University Press, 1991. 333p. $37.50.

The United States and Japan together comprise nearly 40% of the world's gross national product. And the U.S.–Japanese (NichiBei) economy—the sum total of trade and direct investment between the two countries—now contributes more than $150 billion to both economies. Because of the critical importance of the bilateral relationship, its condition and future health are matters of deep concern. Although some in both countries, mainly government officials, continue to argue that the overall relationship remains healthy, there are enough danger signals to warrant close examination of the political economy of the U.S.–Japanese relationship.

Indeed, the problematic growing economic interdependence between Japan and the United States is only one of the more visible manifestations of the collision between the two most powerful social currents of our time: the globalization of local, national economies and the imperative to preserve or enhance cultural or national identity.

Unfortunately, the intellectual architecture necessary to advance our understanding of how "domestic" and "international" political economy forces interact is underdeveloped. Under such conditions, a number of research strategies are appropriate, among them the case study approach used to answer questions that give promise of providing useful guidance for subsequent work. The goal in such work is to use a particular instance as a search strategy to uncover underlying dimensions that may warrant further study. The work under review more than fulfills this goal by providing an efficient description of how Japan responded to the "dollar shock" issue of 1969–71. The author not only weaves disparate material together to illuminate the particular crisis but also succeeds in making a strong contribution to how we should think about the present and future condition of the bilateral relationship.

The issue Angel selected for study presents similarities and differences between 1969–71 and the present. Then, as now, Japan's current trade account with the United States was in surplus. Then, as now, the United States applied pressure on Japan to respond to American demands for Japan to make changes in its international monetary policies. The demands, then as now, were made because of domestic American difficulties. Japanese decision makers proved inflexible and only unilateral American action forced Japan into changes. However, the domestic problems of the U.S. economy are far greater in the 1990s than the late 1960s; and Japan's current account trade surplus with the United States is far greater, as well. In fact, Washington and Tokyo have been negotiating over trade related issues on virtually an annual basis since the 1969–71 crisis, but to little avail. Even after additional yen reevaluations, wave after wave of reductions of tariff barriers, and now agreements by both countries to reduce structural impediments in their domestic economies for mutual advantage, Japan's current account trade surplus with the United States and the rest of the world increases.

The author focuses on the development and implementation of Japan's international monetary policy in response to American pressure to revalue the yen. Angel selects this issue because he questions the "received wisdom" of economists and political scientists, who tend to see national-level economic and political decision making in Japan as smooth, coordinated, and successful. He sets out to (1) identify the participants in the decision-making process, including what accounted for their access in the decision-making process; (2) identify the positions of the participants and how they arrived at their positions; (3) determine who decided Japan's policy response with what level of influence; and (4) decide how all these factors combined to produce Japan's response.

To answer these questions, Angel places the period before the American challenge to Japan in context. By the late 1960s the Japanese had clearly begun to succeed in their effort to catch up with other advanced industrial economies. But their single-minded focus on export-led economic growth precluded much attention, on their part, to Japan's impact on the other societies Japan traded with. Consequently, when the Nixon administration placed a 10% import tax on all imports entering the United States and called for a yen reevaluation, the Japanese government was unprepared to respond. Angel does an excellent job of tracing the Japanese inability to respond to a need for a reassessment of international monetary policies designed to maximize the ability of Japanese companies to compete abroad. He is convincing in arguing that the institutional structures put in place during the American Occupation appear to have been associated with the pattern of Japanese leaders' becoming comfortable with the idea of using foreign pressure (*gai-atsu*) as a means to undertake important domestic reforms. He also notes the somewhat paradoxical legacy of resentment among Japanese elites left by the "dollar shock" case, which was to be only the first in a line of cases in which American policymakers would make unilateral demands and the Japanese would be forced to respond.

The detailed answers to the questions Angel poses is important for comparative political economy theory development in general and state-centered theory development in particular. Japan, like the United States, is a capitalist state; but the resemblance may end there. Angel reminds us that viewing the concept of the state as a social institution anchored in cultural and historical context, rather than as another, albeit powerful, interest group yields important insights about how capitalist states differ. Angel presents a powerful analysis suggesting why it is unlikely that any particular institution in the carefully balanced system of Japanese decision-making institutions will step forward to deal with American pressures in a positive manner. Angel wrote about the 1969–71 crisis, but his analysis is strikingly apt today.

RAND Corporation ROGER BENJAMIN

Grassroots Politicians: Party Activists in British Columbia. By Donald E. Blake, R. K. Carty, and Lynda Erickson. Vancouver: The University of British Columbia Press, 1991. 155p. $39.95 cloth, $19.95 paper.

A dilemma confronts political scientists who attempt to study party politics at subnational levels. On the one hand, a narrative that captures the richness of local politics may contain so much particularistic detail that it appeals mostly to readers who have a specific interest in the locale. On the other hand, a narrative that casts local politics into a broad theoretical scheme may lack sufficient detail to interest readers who are not well versed in macro theories of party politics. To avoid this dilemma requires a careful balancing of empirical observation and theory. Unfortunately, this carefully researched book ends up impaled on the first horn.

The authors seek to explain the pattern of left–right (New Democratic party [NDP] vs. Social Credit) polarization that provincial politics in British Columbia sustained from the inception of a minority Social Credit government in 1952 through the party's 1986 electoral victory under its newly chosen "populist" leader, William Vander Zalm. What accounted for the nearly 40 years of extraordinary electoral success these two provincial parties enjoyed, a run of victories that virtually excluded the Progressive Conservatives and Liberals—the dominant national parties—from the legislative assembly? As their principal data base, the authors analyze the results of mail questionnaires sent to delegates to provincial leadership conventions in summer 1986 (Social Credit), spring 1987 (NDP), and fall 1987 (Liberals), with response rates of 27%, 48%, and 40% respectively. If we accept the authors' claim that notwithstanding these low response rates, usual for mail questionnaires, those who answered are representative of the entire convention population, we can credit the authors with "the first systematic account of party activists at the provincial level in Canada" (p. i).

Indeed, the authors provide some fascinating details about the attitudes and characteristics of provincial party activists. For instance, Social Credit and NDP activists report feeling closer to their party's central group than do delegates to the Conservative and Liberal national leadership conventions. Social Credit activists support government spending that encourages private economic development but split among themselves over social spending in the public sector. New Democratic party activists support government spending and regulation in both the economic and social sectors. Liberal activists seem to adopt positions that are deliberately centrist. While middle-aged males remain dominant in all three provincial parties, union membership among Liberal and Social Credit activists has declined since the mid-1970s. Over the same period, the NDP became more heterogeneous as trade union membership was maintained at nearly 50% but membership in professional associations jumped from 5% to 39%. Over two-thirds of the Social Credit and NDP delegates had attended at least one previous convention, as had nearly 60% of the Liberals. Most activists had been party members for over five years.

The authors characterize British Columbia politics as a struggle between two viable parties, the dominant Social Credit party and its principal opponent, the New Democrats. The SoCreds emerged from populist elements, many of whom were Albertan expatriates with financial and organizational connections to the then successful Social Credit party of Alberta. They combined with antisocialist supporters of the provincial Conservative–Liberal coalition government that collapsed in 1952. The NDP developed from the British Columbia branch of the Cooperative Commonwealth Federation, a national farmer–labor party that promoted socialism through reform. The British Columbia branch, however, contained "a much higher proportion of Marxist socialists committed to fundamental change" than elsewhere (p. 4). This struggle polarized from the 1950s through the 1980s as antisocialist supporters of the centrist national parties, particularly the Conservatives, had to choose between casting their votes for an apparently weak Liberal organization or for the stronger—albeit more radical—antisocialist SoCreds. By the mid-1980s, when the British Columbia Social Crediters no longer ran candidates for the House of Commons, over 91% of the party activists reported voting Conservative in national elections. In contrast, NDP activists, who are required to affiliate with the national party, reported 99% support of the national party candidates. Despite aid from the national party in maintaining a formal presence in most provincial constituencies, the Liberals had not won a single seat in the provincial assembly since 1975.

Borrowing some theoretical ideas from David Butler and others, the authors suggest that the militancy of party activists stands in the way of a more moderate politics. Activists, especially those on the Left, prevent the sort of convergence at the center predicted by the models of party competition developed by theorists like Anthony Downs. Three-quarters of the NDP activists agreed that "there is too much emphasis on public opinion polls and slick advertising in politics today," but so did two-thirds of the SoCreds activists. Similarly, 50% of the NDP activists and 35% of their SoCred counterparts, disdained being "careful not to move too far to the (left/right)" (p. 130). Nevertheless, the authors conclude that the relative flexibility of the SoCred activists "must put the NDP in an inherent comparative electoral disadvantage" (p. 133).

In the fall of 1991, shortly after this book was published, electoral support for the Social Credit party collapsed. In the aftermath of accusations that Premier Vander Zalm had criminally exploited his position for private gain, Social Credit lost not only its majority in the legislative assembly but even its position as the NDP's major opposition. The NDP, led by its "moderate" leader (and now premier) Mike Harcourt, won the government for the first time since the 1972 election, and the Liberals suddenly returned with enough seats to become the official opposition.

Needless to say, the authors' analyses of the attitudes and characteristics of party activists who attended provincial leadership conventions neither anticipate nor provide satisfactory explanations for this turn of events. While readers cannot expect the authors to have predicted Social Credit's imminent collapse, it seems fair to argue that the authors became too imbued with their provincial data. An analysis of why the SoCreds failed in Alberta, for instance, might have provided a perspective on the long-term viability of the SoCreds in British Columbia. Moreover, a broader comparative perspective might suggest that in the age of mass communication, regional parties in modern democracies will decline unless they foster a distinctive ethnic or separatist platform.

Nonetheless, readers with a special interest in local party organization or in Canadian provincial politics will find this book an informative historical study.

University of Cincinnati MICHAEL MARGOLIS

State Organized Terror: The Case of Violent Internal Repression. Edited by P. Timothy Bushnell, Vladimir Shlapentokh, Christopher K. Vanderpool, and Jeyaratnam Sundram. Boulder: Westview, 1991. 312p. $49.95 paper.

Genocide Watch. Edited by Helen Fein. New Haven: Yale University Press, 1992. 204p. $25.00.

The two books are useful and thought-provoking collaborative studies on political repression, state terror, and genocide, practices that have bedeviled the twentieth century and are likely to continue to claim millions of lives unless major efforts are made to prevent such state-imposed death. Yet the respective editors note that their topics, despite their obvious importance, have been neglected by scholars and the general public. The neglect, denial, and distortion of genocide is of particular importance to the contributors to Fein's book, since their focus is on prevention of the most horrible crime that can befall a people. Without recognition of genocide, and an active constituency to oppose this crime against humankind, they believe genocide will continue. Appropriately, *Genocide Watch* is accessible to the general public yet is rigorously argued, avoiding the excesses of either optimism or pessimism. *Organized State Terror,* on the other hand, focuses on how to explain a variety of repressive and violent acts by the state within its own borders. It is a book by and for social scientists, but its quest for understanding grows out of moral passion.

Organized State Terror consists of 16 papers loosely organized under several headings: a theoretical introduction, the structural sources of state organized terror, terror as an instrument of state policy, and the social and political psychology of state terror. Most of the essays are excellent, providing sophisticated analyses of single case studies (and, occasionally, comparative studies) ranging from Argentina, to Cambodia, to the Soviet Union, to Nazi Germany, to Central America, to the totalitarian ministate of Jim Jones in Guyana. In a very real sense, the strength of the book lies in its parts, rather than in any overall theory of why some states, under certain conditions, resort to mass violence against portions of their populations. Indeed, it may not be possible to provide a single explanation of state terror (not to mention, repression and genocide); for, as the case studies make clear, state terror is not a single phenomenon but, at most, a set of family resemblances. It may be possible, however, to find more general explanations for *types* of repression and state terror. Also, perspectives that help locate likely areas of trouble, without offering an explanation in a full sense, can be helpful. Viewed in this light, the editors' "theoretical template" summarizing the diverse case studies might be acceptable. They, however, make a much stronger claim for it, namely, that it can illuminate the general conditions for the emergence of political repression and state terror.

The model consists of four main conditions of repression and terror: (1) a distorted conception of the state and the relationship between the state and society ("When the state views its monopolization of power and authority as absolute, society's institutions loom larger as potentially competing centers of power. The state then operates to preclude that competition." Again, such regimes are "typically animated by ideologies sharply at odds with reality and by unrealistic ambitions for social and economic control" (pp. 10–11); (2) conflict and disarray within the state apparatus, leading to political rivalries and a rhetoric of absolutes—in short, weakness; (3) deep economic and/or ethnic conflicts; and (4) strong state-dependence on a foreign power, so that if established by a foreign power, the state lacks legitimacy and resorts to repression and terror, and if recipient of large amounts of foreign aid for the military and other security forces, it not only has the means for repression but develops a military outlook that emphasizes security at any cost.

Such a model is suggestive—but misleading in that it presupposes that a single theory can explain all examples of state terror. In another sense, the theory is partially misplaced, since it does not attempt to locate and explain *particular* types of family resemblances within the larger category of state-imposed violence. Nor does it indicate whether the four general conditions specified in the model are, singly or in some combination, necessary or sufficient conditions for state terror (repression, genocide). Further, the model offers few hints about the relations between repression, terror, and genocide. Are these different in kind, locations on a continuum, or causal sequences? Is it the case that all states that resort to terror also commit genocide? If they do not, what has prevented them from doing so? Lack of opportunity, external pressure, the effectiveness of terror itself? Unfortunately, these issues are not addressed by the editors. Nor do they attempt to draw upon their analysis of the causes of state repression and terror to suggest ways to prevent the states selective and mass violence against its own population.

The thoughtful essays in *Genocide Watch* by experts in a variety of fields address a number of issues related to the prevention of genocide. None of the contributors has any illusion about moral considerations' prevailing in the calculations of governments either to commit or to deter genocide. Rather, they call for systematic campaigns by nongovernmental human rights organizations to expose genocidal acts and build a constituency that will pressure governments and international organizations to take steps against perpetrator states. They also call attention to the costs of genocide in terms of international and regional stability and security, underdevelopment, aid for refugees, and famine relief. Fein points out that for governments to ignore genocidal acts tells the perpetrators that they can get away with it and details the relocations, massacres, and gassings of the Kurds in Iraq as an example of how the process works.

Yet as Leo Kuper notes, since genocide is not a "unified phenomenon," the possibilities for prevention differ with the "annihilatory contexts" (p. 157–58). In any case, action that is likely to be successful must come in the early stages of persecution.

In this light, several of the essays focus on how to detect genocide, (distinguishing it from, say, civil war or isolated killings as reprisals) and on how medical, demographic, and testimonial information from refugees can help alert us to what is taking place. The press has a major role to play here; but too often, it has—through perceptual bias, fear of offending a government, and distrust of accounts of refugees—ignored, distorted, or only briefly covered major genocides, including Cambodia, the Stalin terror–famine in Ukraine, and Burundi in 1972. Fein reports the argument of Richard Cohen, the columnist for the *Washington Post,* that the press and public will take genocide seriously only if the victims are viewed as innocent, there is an American constituency to support and publicize their cause, and the United States is perceived as having considerable leverage.

The essays identify two other problems that affect the possibility of preventing genocide. One is the need for coordination of the various elements in a constituency for protection against genocide. Although this is a persistent weakness among human rights groups, the report in *Genocide Watch* on the campaign to deter Iran from genocide against the Bahá'ís indicates that fruitful cooperation is possible. The other is that of the bystander who looks on and does nothing. Ervin Staub, in his sensitive and thoughtful study, examines the nature and causes of uninvolvement on the part of the many but argues on empirical grounds that it is possible to transform bystanders into more active, responsible citizens.

Genocide Watch is an insightful and practical guide to the prevention of genocide. Surprisingly, however, only a paragraph is devoted to indigenous peoples, groups that are under constant threat of genocidal attacks. Nevertheless, this is a sobering book, one that should be widely read, and then acted upon.

College of William and Mary ROGER W. SMITH

The Revenge of History: Marxism and the East European Revolutions. By Alex Callinicos. University Park: Pennsylvania State University Press, 1991. 159p. $28.50 cloth, $13.95 paper.

The Left Unraveled: Social Democracy and the New Left Challenge in Britain and West Germany. By Thomas A. Koelble. Durham: Duke University Press, 1991. 162p. $34.95.

These books have little in common beyond an interest in the fate of contemporary socialism. While proclaiming the triumph of Western liberalism as premature, Callinicos' book is a plea to take advantage of the "great opportunity" provided by the Eastern European revolutions for the revival of the Marxist tradition that includes Lenin, Trotsky, and the Left Opposition since that tradition is now freed from its Stalinist incubus and betrayals. For Callinicos, European social democracy does not differ significantly from the conservatism of Reagan and Thatcher, whose liberal economic institutions are still subject to the "deep crises of profitability" foreseen by Marx.

While Callinicos sees the death of Stalinism as an opportunity to revive the spirit of Lenin and Trotsky, Koelble, conceding that the European social democracies are perhaps not too different from their conservative rivals, argues that labor parties are healthy insofar as they have rid themselves of most variants of socialism—Bolshevik or otherwise. Indeed this interesting and careful comparison of the New Left challenge of the 1970s in the British Labour party and in the German Social Democratic party (SPD) shows that any successes they may enjoy are partly due to the triumph of the forces of the "social democratic" Right, as against the socialist Left. Koelble's study indicates that such a strategy has enabled most European labor parties to retain their electoral strength and concludes that social democracy is not the end of the road but is "currently the only viable alternative to the conservative project" (p. 130). This adaptive process is one piece of evidence used by Koelble to show that the elites of the European labor parties are responsive to public opinion, that they disprove Michels's theory of oligarchy, and that the parties and their elites have lost ground by either ignoring new constituencies, like the ecology movements, or taken the old ones (especially the unions) for granted.

While, for Koelble, the overall story of these parties is one of a systematic scrapping of the doctrines of nationalization and workplace democracy and a concentration on softening the harder edges of a market economy, Callinicos scorns theories of market socialism and makes a plea for doing away with the party system as we know it. He envisions a revived system of soviets, a system of workplace democracy that should replace the representative institutions of liberalism, of which the party system is an integral part. Noting the frequency with which soviets have appeared in many of the modern European revolutions, Callinicos argues, quoting Lenin, that such a system is not utopian, "is not anyone's invention," but, rather, "*grows* out of the proletarian class struggle as that struggle becomes more widespread and intense" (p. 111). Callinicos is hardly the first theorist to have observed this phenomenon. While he extols the soviets for their "tendency to break down the distance between economics and politics," Arendt's *On Revolution* (1963), a far more extensive and tragic history (to which Callinicos never refers), sees this breakdown as the basis of the soviets' failure once the necessities of economic housekeeping—better done by bureaucracies—replaces the realm of political freedom in the soviets. Callinicos fails to tell us the conditions under which soviets will arise spontaneously at the turn of the century. He quite effectively condemns the complacency of liberalism and the shortcomings of Keynesianism and predicts that Poland and Hungary will fail to integrate with Western economies and, instead, come to resemble Argentina or Brazil. But since strong soviets have not arisen in Mexico, it is quite another matter to hope or predict that soviets will spontaneously arise in Eastern Europe, not to speak of Western welfare states dominated by citizenries of suburban small property holders.

Why should Callinicos pick fights with social democracy by condemning attempts by Nove, Miller, and others to formulate a "market socialism" (pp. 95) when he himself notes his hero Trotsky's support for the NEP, which was characterized by "various combinations of socialist planning and market mechanisms" (p. 120)? To argue, as Callinicos does, that the forces of history necessitated such "transitional" compromises as a strategy for survival is only to concede the prudence of the European labor parties in making similar political calculations.

Finally, if the history of Western working-class organizations is at all instructive, Callinicos' vision of a relatively decentralized system of soviets and trade unions would be as likely as not to accept market forces and be less concerned to use the power of the state to "force" history. Lenin's condemnation of syndicalism, "tailism," and economism affirms this tendency. It is perhaps significant that the author refers disparagingly to the Stalinist "vanguard" (p. 17), while never linking this term to Lenin or explaining to us adequately why it was Lenin, not Stalin, who was the first to undermine the power of the soviets. Even if soviets were to develop, Callinicos cannot tell us why workers would not be betrayed once again by the state or by an elite of Marxist intellectuals. Whatever Lenin's view, Callinicos' own vision of a soviet future remains utopian.

Koelble's account of the economism of British and West German trade union politics highlights the clash between working-class economism and socialist theory. As Koelble points out, the predominance of British trade unions through a system of bloc membership and voting constituted a large part of the basis of the triumph of social democracy in the postwar Labour party. It was only by ignoring union interests during periods of economic crises and necessary wage constraints, especially by the second Wilson government, that a critical number of unions came to support the New Left calls for controls on the autonomy of the parliamentary party. Since 1983, however, the unions have consistently rejected party executive candidates associated with the New Left and supported the social democratic policies of Neil Kinnock.

The case of the German Social Democratic party is in some respects even more telling. Although by law trade unions are politically neutral and membership in the SPD is individual, the unions, in what can only be called a triumph of economism, rejected ecologist calls for limits on building, on the grounds that they would be harmful to full employment. They regarded the new leftists as cranks and nuisances; and, as Koelble notes, the social democratic countermobilization against them "depended quite frequently on the mobilization of trade union supporters" (p. 92). It is certainly true that much of the thinking in the New Left of both parties cannot be assimilated to the various proletarian ideologies either of old Marxism or of the Western labor movements. Indeed, what are the prospects for a revival of classical Marxism when to one observer, the students and intellectuals of the Left-dominated Young Socialists "wanted to represent the workers but could not understand their language" or, worse, when workers could not understand Young Socialist jargon?

Koelble emphasizes the sometimes agonizing strategic and organizational choices confronting the two parties in the last generation when confronted with the changing constituencies of the various party tendencies and how they dealt with the successionist movements—the Greens on the SPD Left and the Social Democrats on the Labour Right. He addresses the question of why the two parties reacted to circumstances in different ways in regard to coalition building and in regard to responses to very different external events, especially economic conditions. It is a very worthwhile, readable, and valuable study in the best tradition of political science.

Yet we are led to ask whether the work stands as an adequate refutation of Michels's iron law, because the parties responded to outside pressures and to internal bargaining from multiple elites. Michels's (1949) *Political Parties* stipulates that only when elites remain united are they "always victorious" but that when the dominant classes "struck by blindness pursue a policy that strains relationships to the breaking point," the party masses overthrow the party oligarchies (pp. 167, 172). Koelble himself succeeds in showing how Labour blindly ignored shop stewards who precipitated the trade union revolt of the 1970s. By his own account, the party's partial revival is due in part to a ousting of the New Left and the revival of the old PLP–trade union alliance. A similar general conclusion could be drawn from Koelble's account of the SPD, which successfully resisted, through tactics that would have made Machiavelli blush, a New Left takeover (similar to that experienced by the Labour party). In both cases, we are reminded that the more things change, the more they remain the same.

University of California, Riverside JOHN L. STANLEY

Driving a Bargain: Automobile Industrialization and Japanese Firms in Southeast Asia. By Richard F. Doner. Berkeley: University of California Press, 1991. 371p. $39.95.

The role of transnational corporations in Third World economic development is a highly controversial subject, albeit one whose terms of debate are at least coming into sharper focus. Until the early 1980s, there was primarily a "dialogue of the deaf" between modernization theory, which stressed development through laissez-faire economics in which transnational corporations played a key role, and dependency theory, which viewed transnational corporations as the leading actors in perpetuating the "underdevelopment" of the Third World through the exploitative relationships of world capitalism. Since then, the debate has become more nuanced with the realization that significant industrialization in the developing world is possible for both economic and political reasons. Economically, the operation of the "international product cycle" pushes the manufacture of labor-intensive and standardized goods into nations with an abundance of cheap labor. Politically, "strong and autonomous" states in the Third World have been able to exert countervailing power against transnational corporations and thereby to harness their economic activities to domestic development projects.

In *Driving a Bargain*, Richard Doner makes a major contribution to this controversy by presenting a theoretically centered case study of the automobile industry in four Association of Southeast Asian Nations (ASEAN) countries: Indonesia, Malaysia, the Philippines, and Thailand. These ASEAN nations form an excellent basis for a case study of transnation corporate economic power in the "periphery." With the exception of the Philippines, they have had dynamic economies based on export-led drives in the last several decades and have, at least potentially, substantial domestic markets. Transnational corporations, especially Japanese ones, have entered their markets in force. Their governments have shown keen interest in promoting policies of economic nationalism and industrialization. They occupy a distinct niche in the global political economy, trying to follow Japan and the East Asian "gang of four" (Hong Kong, Singapore, South Korea, and Taiwan) up the international product cycle. In respect to the automobile industry, all have had considerable assembly operations generally dominated by Japanese transnational corporations and some automobile parts industries in which local businesses have played a stronger role; and all tried to upgrade their automobile industries by promoting greater local content, industry rationalization, exports, and the role of domestic capital. All experienced some success but fell far short of their stated goals. However, very considerable differences emerged among the indigenous automobile industries in these four nations. Thailand's was clearly the most successful, and the Philippines', the least successful, while Malaysia's and Indonesia's occupied an intermediate position.

Doner presents detailed histories of the evolution of the automobile industry in each country, focusing upon such factors as the mix of local and foreign ownership, the differences between the components and assembly industries, the strategies of transnational corporations (especially their efforts to impede "indigenization"), the political strategies and conflicts of local business interests, regime ties to local and foreign capital, and state policy toward the automobile industry. On the basis of these four case studies, Doner challenges two dominant theoretical conclusions about the transnational corporate role in the developing world: (1) that transnational corporate "structural power" in terms of economic resources and almost inevitable alliances with groups in the host country greatly biases bargaining in their favor; and (2) that states that are strong and autonomous from their societies offer the best opportunity for counteracting transnational corporate structural power.

Rather, Doner advocates a "product cycle" approach. The international product cycle, according to this perspective, produces oligopolistic competition among transnational corporations and the diffusion of technologies that provide opportunities for Third World industrialization and the upgrading of production. The considerable differences among the automobile industries of the seemingly similarly situated ASEAN nations, then, demonstrate that these opportunities are certainly not equally utilized. Doner argues that neither national resource possession nor state characteristics (autonomy and nationalistic policies) explain these variations. Instead, the nature of "growth coalitions" between domestic capital and the state appears to be the key factor in promoting indigenous industrialization. Local businesses provide an insider's knowledge of the industry and a technological sophistication that state technocrats do not possess but that are essential in bargaining with transnational corporations. Conversely, state power is necessary to help local capital expand and pursue its goals vis-à-vis the transnational corporations. Thus, growth coalitions based on "reciprocal consent" between the state and domestic business community are necessary to create the economic and governmental flexibility and strength prerequisite to climbing the international product cycle.

Driving a Bargain, therefore, makes an important contribution to the analysis of the political economy of development by strongly indicating the need to move beyond simplistic images. Regime–society relations emerge as the driving force in development, but they work in complex ways and causal chains. Doner demonstrates convincingly that regime–society relations differ dramatically among even generally authoritarian regimes and that what is important for developmental dynamics is the nature, not the absence, of these links. Thus, political and economic factors are inextricably interwoven, and "development" is an ongoing game of pursuing economic and political self-interest within this matrix. Doner's sophisticated and sensitive relating of case histories to theoretical concerns sets a high standard for understanding this truly political economy of development.

Auburn University CAL CLARK

The Military Revolution and Political Change: Origins of Democracy and Autocracy in Early Modern Europe. By Brian M. Downing. Princeton: Princeton University Press, 1992. 308p. $35.00.

A quarter of a century after European state formation reemerged as a central concern of macroanalysts in both sociology and political science, the controversy goes on. This is no gentle disputation among antiquarians but, rather, a hegemonic struggle among ambitious theorists over the interpretation of our own times. Launched by Barrington Moore, Jr. (*Social Origins of Dictatorship and Democracy,* 1966) as a critique of then-reigning modernization theory, the materialist explanation of regime variation itself stimulated a reaffirmation of the primacy of culture as a determinant of political development (e.g., Bendix's *Kings or People,* 1978). But both sides shared an egregious theoretical flaw, identified as long ago as 1902 by Otto Hintze: because state formation in Europe took place concurrently among many states, interactions among them generated processes that shaped the development of each. Any theory contending that "a people's constitution is in effect shaped by its social structure" was therefore incomplete; and he proposed, as an alternative starting point, the notion that the peculiar character of a given state is "co-determined by its relation to its surroundings" (*The Historical Essays of Otto Hintze* [1975], p. 159). The problem was to ascertain "whether the cases of which this is true are isolated and exceptional, or whether they can be organized in groups and presented as evidence of a typical regular pattern" (p. 160, n. 24).

The theoretical task Hintze delineated is still with us: Although the twentieth century provided plenty of grist for his theoretical mill, by and large, the social sciences remained stubbornly "internalist." In our own discipline, Harold Lasswell's seminal notion of the "garrison state," formulated on the eve of World War II as a "developmental construct" very much in keeping with Hintze's approach, was never incorporated into mainstream theory. However, the internalist hegemony began to wane in the 1970s as a result of two quite disparate challenges. One was "dependency," which argued that a country's economic structures were determined in large part by processes generated at the level of the international political economy. The other stemmed from a congeries of scholars who emphasized war—and strategic interaction among states, more generally—as a major determinant of state formation. Coinciding in time with the exhumation of Hintze, the major work in the latter category was Charles Tilly's compendium, *The Formation of National States in Western Europe* (1975), including not only the editor's own well-known introductory essay but also Samuel Finer's more ambitious attempt to conceptualize military-induced mobilization of human and fiscal resources as a variable and to explore the political consequences of this variation.

Although, in the intervening period, analysis of the war factor has been further elaborated by Tilly and a number of others, Brian Downing lifts us beyond the exploratory phase to a more mature theoretical plane by setting forth a proto-theory of how it contributed to the determination of fundamentally different political outcomes. Although he ignores Finer's contribution, his conceptualization of the factor in terms of resource mobilization is quite similar. But whereas Finer overly elaborated the manifold aspects of the process to the point of producing a jumble from which it is difficult to extract workable hypotheses, Downing adopts the opposite strategy of drastic simplification for purposes of theory construction. However, unlike many other practitioners of comparative historical analysis, he is clearly aware that it *is* an oversimplification and accordingly goes on to examine the historical circumstances that round out the case. The result is an eminently satisfying marriage of theory and history that makes this book as important from a methodological perspective as from a theoretical and substantive one.

Downing's theoretical objectives are highly ambitious. As indicated in his subtitle, he purports to provide nothing less than an explanation of the "origins of democracy and autocracy." His target is clearly Barrington Moore himself and thereby (as for Hintze in 1902) dialectical materialism in its classic form: "I shall argue that constitutional arrangements predating modernization and military dangers in the newly forming state system were at least as important as agrarian relations in determining political outcomes in Western Europe" (p. 9). Remarkably, the contender has obtained from the champion an acknowledgement of his victory by way of a cover testimonial saying, "This book takes a big step beyond my *Social Origins of Dictatorship and Democracy.*"

The first element of Downing's theory refers not to the war factor proper but, rather, to medieval constitutionalism as a baseline condition for the emergence of liberal democracy in Europe. This proposition can hardly be gainsaid, but one must take issue with his insistence that the determinative role of constitutionalism "has been largely ignored by almost all modern social scientists" (p. 5). While Downing himself excepts Randall Collins, Samuel Huntington, and Gianfranco Poggi from the charge of ignorance, many others come to mind, including the late Stein Rokkan; Reinhard Bendix; in the Marxist camp, Barrington Moore himself, who drew from Weber the importance of medieval cities and the bourgeoisie; and Perry Anderson, who emphasized the "detotalization of sovereignty" as a characteristic feature of the West. To these should be added just about everyone writing on the political development of the relevant countries (e.g., the contributors to Raymond Grew's *Crises of Political Development in Europe and the United States,* 1978). Has Magna Carta disappeared from textbooks on British politics?

Downing then proceeds by way of a sort of multivariate analysis, somewhat along the lines attempted by Rokkan, but with a sharper focus on one critical period (the seventeenth century) and six major cases (Brandenburg–Prussia, France, Poland, England, Sweden, and the Dutch Republic). Medieval constitutionalism, as well as the military mobilization factor are treated largely as dichotomous variables, strong or weak. The stronger the constitutionalism, the better its chances of surviving, at least in part, the military mobilization pressures unleashed in the seventeenth century (France and Sweden, as against Prussia). Conversely, constitutionalism's chances of survival are much better if the pressures on it are weak (England) than strong (France). It goes without saying that where medieval constitutionalism is extremely weak or altogether absent (as in Spain, Prussia, and Russia), prospects for the emergence of liberal democracy are practically nil.

But, as noted, Downing shuns reductionism and allows that a number of factors may intervene to break the nexus between war and the destruction of constitutionalism. They include the possibility of mobilizing foreign, rather than domestic, resources (Sweden); lightening of the load by way of alliances (the Netherlands in their war against Spain); the advantages of advanced economy and commercial wealth, if it is in the hands of the merchants (as in Venice or the Netherlands); and, finally, geography and natural topographical features (Switzerland, England, and the Netherlands are easier to defend than France). While there is no gainsaying the significance of these factors, they are brought in on an ad hoc basis and hence lack theoretical import. Although Downing adumbrates possible patterns (e.g., commercial–maritime states are more likely to engage in alliances), he shies from a more systematic account.

Although Downing's framework does not produce startling revelations, it enables him to construct a coherent, theoretically grounded narrative that covers most of the relevant universe and respects historical accounts. This is a major achievement, which points the way to the next theoretical step, namely, an articulation of state formation with the dynamics of the international system of states into a comprehensive body of theory. That is no mean task, not only because weighty configurational elements such as geopolitics are inherently difficult to model but because of interactivity. The war factor is shaped not merely by a given state's responses to exogenous events but also by its own activities at the international level; and these are themselves determined in good part by the general character of the regime. Spain, after all, *chose* to carry out a protracted war against the Netherlands, and Louis XIV *chose* to strive for European hegemony in the late seventeenth century. Were not these projects and the resulting load they imposed on the societies in question a *consequence* of absolutism?

New School for Social Research ARISTIDE R. ZOLBERG

The Struggle for Democracy in Chile, 1982–1990. Edited by Paul W. Drake and Iván Jaksic. Lincoln: University of Nebraska Press, 1992. 321p. $45.00 cloth, $19.95 paper.

Political Structures and Democracy in Uruguay. By Luis E. González. Notre Dame: University of Notre Dame Press, 1992. 224p. $26.95.

These two books make an important contribution to the literature on democratization by focusing on the two countries with the strongest democratic traditions in Latin America prior to their 1973 coups. They are concerned, however, with two different questions. The book edited by Drake and Jaksic focuses on the problem of transitions from authoritarian rule, while González's book is primarily about the prospects of democratic consolidation in Uruguay since 1985. In spite of this difference, there is an interesting convergence between the theoretical approach advanced in both books.

The Struggle for Democracy in Chile, 1982–1990 picks up the story of Pinochet's Chile where J. Samuel Valenzuela and Arturo Valenzuela's edited volume *Military Rule in Chile* (1986) left off. It consists of 10 articles, written by an outstanding international group of country specialists, that concentrate on the changes both within the authoritarian regime and in society in the wake of the first serious demonstrations against the Pinochet-led regime in 1983. The accounts go through the inauguration of a democratic administration in March 1990. The introduction by the editors outlines in broad strokes the transformations between 1982 and 1990. A superb article by Arturo Valenzuela describes the institutional structure of the regime, highlighting the personalistic control exercised by Pinochet. Varas's essay, focusing on tensions within the regime, supplements this account. Two more essays round out the study of the authoritarian regime. Campero's focuses on the role of a firm group of supporters of the regime, that is, the entrepreneurs. Silva's focuses on the government's response to the 1981–83 economic crisis. The next set of three essays studies different aspects of the opposition to Pinochet, including the role of women (María Valenzuela), trade unions (Angell), and political parties (Garretón). The volume closes with a look at external factors (Portales) and the economic legacies inherited by the new democratic government (Larráin).

In *Political Structures and Democracy in Uruguay,* González, a Yale-trained Uruguayan political scientist, provides an analysis of the effects of politicostructural factors on democratic stability. He briefly describes the historical evolution of the party system and the executive branch and explains the 1933 and 1973 breakdowns of democracy. Thereafter, the bulk of the book analyzes the prospects of democratic consolidation in the new democracy restored in 1985. Central to the elaboration of this analysis are two surveys, which constitute the book's main empirical contribution. One is a 1985 public opinion survey, the other, a 1986 survey of legislators, which allows González to describe the complex nature of party factions and the attitudes of political elites. The book's central argument is that the 1973 breakdown was influenced by the political structures that had been in place for over a half a century, namely, by the fractionalization of the major parties and the "quasi-presidential" system. Concerning the prospects of democratic stability after the 1984 election, which brought an end to military rule, González argues that the factors leading to the 1973 breakdown have, for the most part, remained in place through 1990. Because the 1984 election saw a true restoration of the party system, there was a resurgence of many of the problems that facilitated the 1973 coup. The analysis, in short, leads to a negative reading of Uruguay's democratic future.

Theoretically, these two books exemplify what could be called a "political-institutional" approach to politics. The need to stress purely political factors is most explicitly elaborated in González' book, which resembles Arturo Valenzuela's *Breakdown of Democratic Regimes: Chile* (1978) in advancing a classically Linzian analysis through a case study on democratic stability. The nature of the electoral system and the party system, along with the nature of the executive power, are thus viewed as political structures that actors strategize in and that have a crucial impact on the viability of democracy. Furthermore, searching for a solution for Uruguay's uncertain democracy, González presents an argument similar to Linz's concerning the problematic effects of presidential systems from the perspective of democratic stability. The best remedy, in short, is seen as a shift to a parliamentary system. The problem, then, is that the necessary institutional changes face impressive obstacles. The issue of democratic agency, thus, is seen as going beyond the commonsense notion that political actors do things that undermine democracies, to include the broader question of how to transform political structures in such a way that they should ensure the apparently contradictory goal of democratic stability. The democratic challenge is to shape a political arrangement that allows and invites political participation while securing its stability against corrupting tendencies. In focusing on the actions of political elites and considering the strategic options they face, the book's conclusions are quite pessimistic in the end.

González's contribution, in short, is to have provided a detailed study of an understudied case and framed it in terms of the ongoing debate among comparativists, spearheaded by Linz and Lijphart, on the political institutions of democracy. The book's main shortcoming is that the explicit focus on political elites is not balanced through the study of actors in civil society. As in much of the "political-institutional" literature, questions about the type of linkage between political parties and social actors (e.g., business, labor, and various social movements) are never addressed. An issue as basic to democratic theory as whether there are actors with well-defined interests that can then be represented, therefore, simply does not emerge.

If González's main concern is with democratic stability, the volume edited by Drake and Jaksic focuses on the other side of the democratic question. Rather than considering how to stabilize a new democracy, they study the antecedent process of moving away from authoritarian rule. Their concern is thus with factors that lead to the destabilization and removal of an authoritarian regime. Along the lines of O'Donnell and Schmitter's classic *Transitions from Authoritarian Rule* (1986), this book shows that in transitional periods, actors seem to enjoy a greater scope for choice than in periods of "normal" politics. Also consistent with most of the transitions literature, the number and type of actors considered in Drake and Jaksic's book suggests that in fluctuating scenarios, a broad range of actors engage in political action. The plurality of actors, nonetheless, converge into neatly divided camps, which imprint, to a greater extent than in democratic systems, a peculiar state–opposition dialectic upon the process of transition.

In contrast to O'Donnell and Schmitter, however, action and reaction are viewed as framed by institutional factors or political structures. Thus, in a manner consistent with González' approach, Arturo Valenzuela and Garretón, in particular, among the contributors, show how the strategies of various actors cannot be understood outside of the institutional characteristics of the regime. It follows from this that transitions are in part determined by the peculiar institutional features of the preceding authoritarian regime. Again contra O'Donnell and Schmitter, who sought to find the common elements shared by all transitions, this approach leads to a highlighting of variations in the process of transition. This emphasis on strategic actors operating within political structures, in short, contributes to a key aim of comparativists, namely, to distinguish between different "modes of transition." The next question in comparative analysis, however—the possible links between variations in the transitional process and the problems and prospects of democratic consolidation—is beyond the scope of the Drake and Jaksic volume. The research of this question, moreover, would necessitate a move beyond these two works. It would require an explicitly comparative study, and it would demand consideration of cases both in their phase of transition from authoritarian rule and as new democracies. These two books, nonetheless, are essential readings for the advancement of such a research agenda.

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Revolutions of the Late Twentieth Century. Edited by Jack A. Goldstone, Ted Robert Gurr, and Farrokh Moshiri. Boulder: Westview, 1992. 395p. $55.00 cloth, $19.95 paper.

Autocracy, Modernization, and Revolution in Russia and Iran. By Tim McDaniel. Princeton: Princeton University Press, 1991. 239p. $29.95.

Under the impact of the world's enormously varied violent transfers of state power since 1945, political analysts are finally relinquishing an old illusion, namely, that revolutions are singular phenomena for which it is possible to state a single invariant set of necessary conditions, sufficient conditions, and internal sequences. As with the abandonment of many old, cherished beliefs, the new apostates are not always aware of how heterodox they have become; and those who do know often only whisper their unbelief. So it is with Tim McDaniel, Jack Goldstone, Ted Gurr, and Farrokh Moshiri. They broadcast many other messages but mute their apostasy. In vivifying a view of revolution as contingent and various, they have nevertheless mortified the classic sociology of revolution.

At first glance, Tim McDaniel looks like one of the old believers, arguing (as he does) the common properties of revolutionary processes in Russia and Iran. In earlier work, he presented the Russian Revolution as an outcome of contradictions between state-initiated capitalist industrialization and the maintenance of czarist autocracy. Now he claims that the revolutions of 1917 and of 1979 both originated in autocratic modernization, a path to the modern world Barrington Moore's "conservative modernization" did not quite capture. Unlike Germany's Junker-based authoritarianism, says McDaniel, the prerevolutionary Russian–Iranian drive toward industrial prosperity rested on the arbitrary intervention of a divinely sanctioned or self-sanctioned ruler in a country's economic affairs. Thus, the conditions for revolution in those two countries differed deeply from those that prevailed in other kinds of states, where either old elites or the new classes formed by capitalism made their weight felt.

In a dualism resembling Samuel Huntington's portrayal of Western revolutions, that intervention simultaneously undermined the old bases of stability and inhibited the creation of new ones. Autocratic modernization hollowed out society, leaving a space into which disorder rushed. To present this view, McDaniel makes an unfortunate choice, writing a series of essays to emphasize differences and (chiefly) similarities between Russia and Iran, rather than providing two separate sketches, each linked internally by a coherent causal analysis. The result is a provocative set of comparisons, rather than a compelling parallel analysis of revolutionary processes.

In order to make his analysis plausible, McDaniel offers some dubious historical judgments. He greatly exaggerates, for example, the "hallowed tradition" and "divine sanction" of Russian autocracy, forgetting that 14 serious pretenders gained popular support during the seventeenth century, that great families attempted to place their own kinsmen on the throne repeatedly when a disputable succession or a weak ruler arrived, and that a half-dozen czars died at the hands of assassins. In the case of Iran, he minimizes the importance of *bazaari*s and mullahs as connected social elites before and during the revolution of 1979, treating the bazaar's tightly linked merchants as residues of the past, rather than constituents of modern Iranian social life.

McDaniel likewise shows surprising indifference to geopolitical, fiscal, and military effects, to which the works of Theda Skocpol should have sensitized him. On the Russian side, for example, the Russo–Japanese war and World War I disappear as effective causes of revolution. On the Iranian side, we read almost nothing about the shah's close alliance with the United States, except that Jimmy Carter's promotion of human rights made the monarch more vulnerable to claims from dissidents within Iran. Skepticism in these regards does not automatically vitiate an analysis of the Russian or Iranian revolutions. Still, others have made strong enough cases for the importance of such external links in these, and many other, revolutions to warrant the skeptic's ruling them out explicitly, rather than simply ignoring them. Despite the blind spot, McDaniel's interesting version of Alexis de Tocqueville, Emile Durkheim, and Samuel Huntington offers an important challenge to the next round of revolutionary analysts.

When they are speaking most generally, Gurr, Goldstone, Moshiri, and their contributors voice ambivalence about the universality of revolutionary processes. They argue that since 1945 the international situation has changed so much that revolutions occur in different kinds of countries, with different mobilizing claims, and under different circumstances. Yet they also argue that "all revolutions proceed through three phases, beginning with a state crisis, followed by a struggle for power, and efforts by the eventual victors to reconstruct the state. Moreover, no revolutionary crisis occurs until there is a conjunction of three necessary conditions: a crisis in the old state, alienation of a significant segment of the elite, and mass mobilization" (p. 325). The apparent contradiction disappears, however, when we recognize that the universals follow tautologically from the definition of revolution as "the forcible overthrow of a government followed by the reconsolidation of authority by new groups, ruling through new political (and sometimes social) institutions" (p. 37). In fact, the bulk of the Gurr–Goldstone–Moshiri book faithfully documents the diversity of revolutionary conditions, processes, and outcomes in the contemporary world, as well as their difference from revolutions of the past.

The compendium of cases will serve students of twentieth-century revolutions well: Vietnam (H. John LeVan), Nicaragua (Dévora Grynspan), Iran (Farrokh Moshiri), Poland (Jaroslave Piekalkiewicz), Afghanistan (Anwar-ul-Haq Ahady), the Philippines (Richard J. Kessler), Cambodia (Barbara Harff), Zimbabwe (James R. Scarritt), South Africa (C. R. D. Halisi, Patrick O'Meara, and N. Brian Winchester), the Palestine Liberation Organization and Intifada (Joshua Teitelbaum and Joseph Kostiner). All are reflective, informative, and connected somehow with the Goldstonian scheme presented at the book's outset. Introductory and concluding essays by various combinations of the editors neatly stitch the book and its cases together.

The book disappoints, nevertheless, precisely where its conceptual preferences should have made it strong, namely, in specifying how the international system changed the conditions affecting revolutions after 1945—for example, the links (however indirect) between the Cold War and the rise of nationalist revolutions. Those links inevitably appear, disguised, in the accounts of individual countries, as in Richard Kessler's treatment of Ferdinand Marcos's connections to Ronald Reagan and George Bush. Jaroslaw Piekalkiewicz' analysis of Poland likewise inevitably takes up Soviet intervention. Yet despite acknowledging that in all cases "international factors have contributed directly or indirectly to state crisis" (p. 331), the Gurr–Goldstone–Moshiri "comparisons and policy implications" deal almost entirely with domestic politics. These authors provide no reasoning that leads to inferences concerning the impact of the Soviet Union's disintegration (or the European Community's integration) on the prospects for revolution in Europe or elsewhere. The theoretical revolution, it seems, has not quite completed its work.

New School for Social Research CHARLES TILLY

Red Vienna: Experiment in Working-Class Culture, 1919–1934. By Helmut Gruber. Oxford: Oxford University Press, 1991. 270p. $29.95.

In this work, Gruber has chosen to study a fascinating experiment in which politics and culture were linked in an effort to create social change. Picking up where Carl Schorske's *Fin-de-Siecle Vienna* (1980) leaves off, the book begins with an insightful description of Vienna as a socialist laboratory—a thriving metropolis ready for social, political, and cultural experiments. Gruber then proceeds to discuss the policies and programs implemented by the socialists between 1919 and 1934. These programs involved everything from reforms in public housing and social services to dramatic attempts at redefinition of family life, community spirit, artistic taste, and use of leisure time. He examines the real accomplishments of these programs, as well as the cultural and political obstacles which ultimately undermined the Viennese experiment.

The core of the book is Gruber's analysis of the Viennese socialist cultural programs. Here he brilliantly discusses the elite culture and cultural theory, the power of the written and spoken word, the attempts to enrich and ennoble the workers' artistic taste, and the inevitible conflicts arising from the desires of the party leaders versus those of their working-class supporters. His interpretation is balanced and insightful, acknowledging that although serious problems existed in the conception and implementation of policies, red Vienna actually was quite successful in implementing several innovative and positive social reforms.

Gruber's book is exhaustively researched and well written. Taking almost five years to complete the manuscript, Gruber has managed to assemble a wealth of material from a variety of sources, including new documentation from Viennese archives and university institutes. Of particular interest is his research regarding women workers. Even though women made up approximately 40% of the labor force at the time, few treatments of this period have dealt with the recruitment of women to the socialist party and the party's attempt to address women's issues as part of their cultural programs. Therefore, Gruber's research and analysis are a very welcome addition to our understanding of this subject.

Overall, this work skillfully integrates political, social, and cultural history. The book represents an important contribution to the study of Austrian social democracy and provides a useful case study for students interested in cultural politics, urban renewal, and social reform.

Hamline University KAREN J. VOGEL

Authority without Power: Law and the Japanese Paradox. By John Owen Haley. New York: Oxford University Press, 1991. 258p. $37.50.

Political Life in Japan: Democracy in a Reversible World. By Takako Kishima. Princeton: Princeton University Press, 1991. 142p. $27.95.

Few studies of Japanese politics have yet penetrated into the mainstream of comparative politics. Too frequently driven by the desire to explain yesterday's headlines using a Japanese terminology completely incomprehensible to non–Japan specialists, most works on Japanese politics remain theoretically barren and therefore, for most comparativists, are consigned to those small compartments of their brains reserved for "exotica." This has begun to change, fortunately, in the area of political economy. There, for at least a decade, both Japan specialists and non–Japan specialists have been engaged in a theoretically rich and highly informative set of exchanges. It remains to be seen whether that experience will be carried forward into other dark corners of Japanese politics; but clearly the burden is on the Japan specialist to create works that become empirically and theoretically indispensable to nonspecialists.

The two books under review endeavor to reach such a wider audience, albeit with varying results. On the surface, they would appear to have little in common aside from the fact that both deal with Japanese politics. Yet essentially, both argue that the surface-level order that seems so pervasive throughout Japan is ultimately quite precarious. Haley, drawing on a range of secondary literature, offers a lengthy, historically rich examination of the development of Japanese law. Japan, he argues, appears to have a centralized state with a high level of authority that enables it to intervene in all areas of life. Yet, in reality, the government's de facto capacity to coerce and compel is remarkably weak (p. 14). Consequently, Japanese governments from prefeudal time to the present have continually been forced to rely on "extralegal, informal mechanisms of social control as a means for maintaining social order with a concomitant transfer of effective control over the rules and norms that govern society to those who are able to manipulate these informal instruments of enforcement" (ibid.). In short, the real power of social control rests with families, villages, firms, and other small social units.

Kishima is also concerned with the power of those below. But in contrast to Haley, she is far more concerned with demonstrating the applicability of poststructuralist theories to Japanese political life than with exploring inherently problematic aspects of the Japanese experience. Drawing heavily on Foucault and the vocabulary of deconstruction, her short book offers mostly firsthand examinations of what she calls "marginal beings and liminal states" in contemporary Japanese politics (p. 22). Doing so, she contends, allows her to illustrate the "human, non-reified interactions among political players." In practical terms, this translates into an examination of several cases: a parliamentary Finance Committee chairman's subtle and informal human relations massaging of the committee's members; an examination of the "orgiastic atmosphere" of an election campaign headquarters; nonsensical dancing and play acting by politicians at the dinner party; a tearful presentation by former prime minister Nakasone before the ruling Liberal Democratic party's executive board; and portraits of three of that party's officials seen as outside "tricksters," defiant of established political rules. This combination of microstudies, she contends, illustrates how Japanese politics can take on subjective meaning to its participants and, more importantly, how such activities provide a basis for "dynamic, progressive, and irreversible social change" in the country at large (p. 14).

Haley's book is by far the more explicitly comparative. Indeed he uses the comparative method to take direct aim at a number of longstanding cliches about Japan. Perhaps his most important target is the notion that the United States offers some logical comparative standard by which to measure Japan. In law, as in most other areas, he notes, the two countries have far less in common than Japan has with most parliamentary systems of Western Europe. In this context he also makes a good case against the notion that Japanese are culturally nonlitigious. Rather, he shows, the country's limited number of lawyers, judges, and procurators make the Japanese court system one of Japan's least efficient organizations. Why bring a case when it may be a decade before it is decided?

In addition, Japanese crime rates are applaudably low and declining in comparative terms; but little of this, he demonstrates, is the result of tough cops, tight laws, and merciless judges. Instead, almost infinite discretion by police, procurators, and judges is the rule in Japan; and this is aimed at "correction" of criminal behavior, rather than its punishment. Japanese-style correction involves engaging the criminal, the victim, and their respective social organizations (usually family or firm) in the process of creating remorse and restitution by the criminal, which in turn reduces recidivism in Japan to admirably low levels.

Still another major thread in Haley's study involves the demonstration that while their formal powers appear to be great, contemporary Japanese bureaucrats derive most of their formal powers from rules put in place by the U.S. Occupation, not from any Japanese tradition of statism. Moreover, he argues that bureaucrats rarely have the power simply to give orders to the objects of their alleged control: "The formal regulatory powers that Japanese administrations do exercise are . . . notably weaker than those of comparable governmental agencies in other industrial democracies" (p. 159). Instead, he argues (following Richard Samuels), most typically bureaucrats must bargain to ensure compliance with their goals.

Finally, in a very compelling interpretation, Haley contends that the power of Japan's organized crime families reflects the general weakness of authority in Japan, demonstrating instead the importance of autonomy and self-policing by powerful organizations. Ultimately, therefore, he contends that Japanese law "serves as a means for legitimating norms while it remains relatively ineffective as an instrument of coercive control" (p. 200). And the norms (at least in the many marginally legal areas of Japan where organized crime families are influential) are those that favor allowing the criminals to police themselves.

Haley's book is by far the easier of the two to follow in its central argument. With its one central thesis, it musters a series of easily comprehended examples as illustration and thus resonates well with existing standards of social science. The empirical examples in Kishima's book, on the other hand, are more sparse. While generally interesting and informative about the day-to-day human aspects of Japanese politics, most of her examples are presented principally to illustrate a much more ponderous theoretical architecture that in the end remains far from compelling. Delightfully humane instances of human interaction are freighted with the heavy deconstructionist burden of proving far more. Perhaps, for those so inclined, the cases offer major new insights into human self-fulfillment and social change; to others they will be far more valuable as portraits that show in convincing human terms that Japanese politicians are not terribly different from their counterparts in other political venues.

Commitment, or noncommitment, to the deconstructionist paradigm will be the ultimate test of how Kishima's book is received. Poststructuralists will surely find it a fine example of the cross-national applicability of their theory. Doubters are less likely to be convinced. Haley's book, on the other hand, has by far the greater chance to achieve broad appeal, because it engages a series of issues that both are currently very salient to a broad spectrum of scholars and at the same time involve central puzzles that are applicable in any industrialized country. It is in that direction, surely, that the future integration of Japanese studies into comparative politics must lie.

University of Colorado, Boulder T. J. PEMPEL

Behind the Intifada: Labor and Women's Movements in the Occupied Territories. By Joost R. Hiltermann. Princeton: Princeton University Press, 1991. 265p. $29.95.

The Palestinian Uprising: A War by Other Means. By F. Robert Hunter. Berkeley: University of California Press, 1991. 292p. $24.95.

Both of these studies examine the origins and staying power of the Palestinian Uprising that broke out suddenly in December 1987 and continues in various forms today. Both authors contradict two commonly held but mistaken views of the Intifada, namely, that it was a spontaneous outburst of the masses lacking both organization and leadership and that it was the work of local agents of the Palestine Liberation Organization following orders from outside the occupied territories. These explanations assumed that the revolt lacked strong roots in Palestinian society and that it would exhaust itself after an initial burst of unguided fury. Instead, Hiltermann and Hunter persuasively portray the Uprising as the culmination of a decade-long process of association formation and political mobilization that transformed the character of the West Bank (and to a lesser extent the Gaza Strip), despite repeated crackdowns by Israeli military authorities.

Both authors highlight the years of institution building that preceded the Intifada, carefully tracing the emergence of a diversified network of voluntary groups among workers, students, women, merchants, professionals, civil servants, villagers, Muslims, and Christians. Although these groups did not initiate the uprising, they all joined in quickly providing it with organization, leadership, and coordination. In turn, the Intifada gave many struggling and fragmented groups an infusion of new members and a common set of goals. Before long, the art of association became so closely intertwined with nationalist protest that they were virtually indistinguishable.

Hiltermann and Hunter agree that the growing vigor and resourcefulness of Palestinians' associational life has had far-reaching consequences in and beyond the Occupied Territories. The Uprising was sustained and periodically renewed despite recurrent pronouncements that it had run its course. A whole new tier of "inside" leadership emerged alongside the old guard of the PLO, strengthening Yasser Arafat's claims of preeminence while pushing him toward the more urgent pursuit of a negotiated settlement and a two-state solution. Israeli public opinion was forced to reconsider the costs of continued occupation and the temptations of creeping annexation. American diplomacy was given yet another opportunity to take up the unfinished business of the Camp David accords.

Most important, both writers interpret this florescence of associational activity as convincing proof that Palestinians in the Occupied Territories are capable of representing themselves. Hiltermann and Hunter share remarkably similar views of the historical significance of this change. Their conclusions are nearly identical on four major issues.

First, rapid group development signifies at least the partial resuscitation of civil society in the Palestinian heartland. After experiencing multiple traumas, the sinew and tissue of community have grown back to the point where they can support a stronger awareness of common sentiments and a more complex division of labor. Although neither author uses Durkheimian vocabulary, they furnish vivid descriptions of a society where both "mechanical solidarity" and "organic solidarity" are increasing at the same time.

Second, voluntary groups have become the nucleus of the Intifada and the base of a more powerful national liberation movement. Most groups are allied with one of the competing factions that have fragmented Palestinian politics for decades, particularly with Fatah, the Democratic Front for the Liberation of Palestine, the Popular Front for the Liberation of Palestine, the Communist party, and various Islamic groups. At times, this linkage encouraged the proliferation of weak associations, squabbling over the same limited membership. On the other hand, these same connections also invested interest group leaders with important leverage in fashioning new alliances between the factions. Association leaders in the Occupied Territories not only took up key positions on the steering committee of the Intifada, they also pressured the factions to cooperate with one another on the "inside" and the "outside." The result was a more unified PLO with a wider mass following but also with greater responsiveness to constituencies it formerly took for granted or sought to control.

Third, the group network is frequently described as the blossoming infrastructure of a future Palestinian state. This may be the boldest and most debatable conclusion of both books. It is one thing to argue that the "institutionalization" of the Uprising symbolizes a maturation of Palestinian society and a deepening of its nationalist consciousness. It is quite another thing to suggest that these organizations comprise a state-in-the-making.

Hiltermann and Hunter leave little doubt that the people of the Occupied Territories have cultivated the art of association well enough to represent themselves in several arenas at the same time—locally, regionally, and internationally. Indeed, their achievements have far surpassed the expectations of the PLO and Arab patrons who financed community organizations with every confidence that they would serve their sponsors' interests more than their own. Nevertheless, the authors engage in wishful thinking when they suggest that a vigorous associational life is a way station on the road to an independent state. Yet even this exaggeration has merit if it alerts us to the possibility that a Palestinian state might be politically pluralistic and competitive and thus not easily manipulated by either its leaders or its neighbors.

Fourth, both writers agree that the Intifada is not a social revolution within the Palestinian community. Rather, they note that pressures to sustain a united front during the uprising have postponed demands for social justice accumulating for years among workers, women, youths, and the poor. Hiltermann is much more critical and partisan than Hunter on this issue. Hiltermann frequently disparages the conservatism of Fatah and the "bigotry" of Islamic groups while praising the progressivism of the PLO leftists and the communists. He is concerned that the uprising may actually reinforce some of the most repressive aspects of Palestinian culture. Hunter, on the other hand, notes a more benign desire to preserve a wide range of popular traditions, including customary law, arts, and religion. Hunter gives an informed and subtle account of the contribution of the Islamic movement to the Intifada that is refreshingly free of the usual polemics and stereotypes.

Both books are valuable supplements to earlier studies of West Bank and Gaza politics by writers such as Emile Sahliyeh and Ann Lesch, who focused more on elites and officeholders than on associations. Hiltermann is a sociologist who concentrates on the details of the labor and women's movements whereas Hunter is a historian who tries to situate social change in grand contexts, temporal and international. Neither study contains much in the way of social theory. Both are highly readable, mainly journalistic accounts that wisely allow many interviewed participants to speak for themselves. Hiltermann's voices are mostly left-wing organizers, whereas Hunter's are mostly fellow university professors and intellectuals. I strongly recommend listening to both choruses.

University of Chicago ROBERT BIANCHI

The Political Economy of National Security: A Global Perspective. By Ethan Barnaby Kapstein. Columbia: University of South Carolina Press, 1991. 252p. $39.95.
The Political Economy of Defense: Issues and Perspectives. Edited by Andrew L. Ross. Westport, CT: Greenwood Press, 1991. 240p. $47.95.

The combination of the Reagan military buildup and its consequences and the ending of the Cold War has inspired a recent burst of publications on the political economy of national security. *Political economy* is a fashionable term these days and, as such, has become difficult to define. It encompasses quite a broad range of research in both approach and substance. At times, it includes far more than it excludes. Within the broad and somewhat nebulous category of political economy, what is the political economy of national security? To judge from these two volumes, the political economy of defense includes any analysis of the interactive relationship between politics and economics in the area of national security. For Ethan Kapstein, at the core is the fundamental and enduring question of nations' simultaneous pursuit of security and prosperity. According to Andrew Ross, who edited *The Political Economy of Defense*, it encompasses three areas of research: "(1) the political dimensions of economic actions in the defense realm; (2) the economic dimensions of political actions in the defense realm; and (3) the security dimensions of political economic actions" (p. 8). Despite the apparent parity among the three dimensions, the fundamental and dominant concerns are largely economic, including the economic consequences of defense spending, the economics of international arms trade, defense industries, fiscal constraints, and defense planning.

These two books share more than an emphasis on political economy. Both volumes are comparative and international in approach. The Kapstein text explicitly argues that the political economy of national security can only be understood as an interaction of national and international forces and that this is increasingly the case in an interdependent world economy. It also uses comparative cases throughout, from both developed and developing countries. The chapters in the Ross volume, while sometimes focused on one country, tend toward comparative and international analysis, as well.

While the books begin from common concerns and subject matter, they diverge in their merit as scholarship and in their utility for the classroom; that is, the Kapstein volume is a relatively succinct textbook, while the Ross volume is an edited reader, and each enjoys the benefits and drawbacks of their respective formats. The Kapstein book is comprehensive in scope but of rather shallow depth. The Ross volume has sufficient depth in many chapters but is like many other edited volumes a somewhat eclectic selection of topics and contrasting approaches.

As a textbook, the Kapstein volume is a solid piece of work. It is nearly up to date (the best that can be expected of anything published today in international relations), comprehensive in its coverage, and strengthened by the comparative and international perspective. In good textbook fashion, it touches on most of the major issues on the economic side of national security, including chapters on defense spending, defense industries and procurement, the arms trade, economic relations among military allies, and the effect of a global economy on national security. Most of the discussion is well done and informative. Kapstein's emphasis on the effects of interdependence, including the internationalization of defense production, is especially interesting. But two drawbacks are evident.

First, the breadth of coverage is not matched by sufficient depth. Several of the chapters left me (imagining myself as a student) wanting more—much more in some cases. Only a few of the chapters (e.g., that on the arms trade) seemed to provide enough substance and detail. Good students, I think, will be left unsatisfied. One chapter on mobilization, war, and conversion, for example, was weighted more toward mobilization than conversion, although the latter, I think, is the more controversial and more interesting problem these days.

Second, the author's initial analytic framework and subsequent claims about frameworks are handled somewhat loosely. The introduction offers the obligatory tour of the major paradigms in International Political Economy (liberalism, realism or neomercantilism, and marxism); but there is a lack of clarity about whether students are to use them as explanations of, or prescriptions for, state behavior. Marxism is described as an explanation (it is not, of course, prescriptive). But Kapstein ends the introduction by saying the best policy prescription is a combination of liberalism and realism. From there on, the IPE trinity is not used systematically. And much of what Kapstein talks about does not necessarily fit those models anyway—such as weapons procurement and defense spending, where national politics and bureaucratic forces are important. This makes the IPE models look rather obligatory and pro forma. Kapstein's claim that he wants to provide a framework for analysis in each chapter only underscores this problem. Frameworks rarely emerge clearly (the chapter on alliances is an exception). Most of the time, one has been given a good introduction to the topic and provided with some concepts and examples.

The Ross volume seeks to transcend the traditionally segregated political and economic approaches to the study of national defense. In his introduction, Ross makes a tentative case for the *political economy of defense* as a nascent and potentially coherent subfield; but this attempt seems strained. Just as political economy is a collection of theoretical approaches and substantive concerns, so is the political economy of defense. Consequently, the merit of the volume is in the individual chapters, not in its effort to define an emerging endeavor. As the title and author note, it is an assemblage of issues and perspectives. Moreover, the kind of work in the Ross book has been going on for quite some time. Somewhat uneven in their quality, most of the chapters are interesting and informative; yet all in all, they offer no surprises or innovative approaches and insights. Most of the findings are familiar or self-consciously inconclusive.

Although the editor argues that the political economy of defense transcends a narrow focus on the economic consequences of military spending, 5 of the 10 chapters address that problem directly; and it is an indirect concern in 1 or 2 others. For example, Steve Chan provides a brief literature review on the relationship between military spending and economic performance; and Daniel Nelson examines the impact of Warsaw Pact defense spending on member nations. Much of this work is substantial and well-done research, such as Judith Reppy's comparative piece on the relationship between military research and development funding and international trade performance and David R. Davis and Steve Chan's longitudinal analysis of security and welfare in Taiwan. The most political analysis is the chapter on electoral cycles and defense spending in Israel, by Alex Mintz and Michael D. Ward. Politics is also important in Glenn Fong's very good chapter on Pentagon–industry collaboration in high technology.

While one doubts that the political economy of defense is more than a broad set of issues and perspectives, that is not an argument against its importance as an area of research. I do have a concern about the apparent increase in economy-centered research in the area of defense and national security now that the Cold War is ending. My concern is that the strong tilt toward economic analysis and economic issues could crowd out real politics and political analysis of military policy, in much the same way that the dominance of strategic logics (e.g., deterrence theory) in international relations theory tended to preclude the insights of political analysis in our understanding of national security policy during the Cold War.

University of California, Santa Cruz Daniel Wirls

From Revolutionary Cadres to Party Technocrats in Socialist China. By Hong Yung Lee. Berkeley: University of California Press, 1991. 437p. $48.50.

Hong Yung Lee's new book is an extremely broad study of "the structure, personnel, and historical formation of the cadre system in China" (p. 1). It covers the entire history of the Chinese Communist party (CCP) from its founding in 1921 to the present although the bulk of the material deals with the post-Mao reform era. An immense amount of data, including 56 tables, are presented on all levels of the Party and state from the highest officials in the land to ordinary cadres and Party members. The substantive issues covered include recruitment patterns, bureaucratic methods of personnel management, the politics of cadre staffing, and speculative argument concerning the characteristics of the political system emerging during the reform period. While readers will undoubtedly find something of interest in this wide-ranging study, it has significant limitations in terms of both empirical accuracy and conceptual clarity.

Although the book's central theme that there has been an elite transformation from "revolutionary cadres" to "Party technocrats" is clearly stated in the title and is, within limits, noncontroversial, it is more difficult to determine its basic argument in terms of how elite composition relates to political outcomes. Perhaps the most representative statement of the author's position is that "the less institutionalized a political system [like China's], the more likely political elites will bring the ideology, experiences, and outlooks of the social classes from which they came into the political process" (p. 387). There are several problems with this which can be seen in Lee's treatment of the revolutionary cadres dominant in the Maoist era. First, Lee ascribes an alleged "continuing rural orientation of the revolutionary elite" after 1949—itself a questionable notion—to the initial recruitment of the ruling elite "largely from poor peasants with a low level of education" (pp. 47, 387). This is simply not so if we regard the "ruling elite" as most concretely represented by the Central Committee elected in 1956. As Donald Klein's work 30 years ago demonstrated, these leaders were far from the bottom of the traditional social scale in their origins and they were quite well educated with substantial numbers having tertiary, and even foreign, training. But perhaps *ruling elite* is meant to refer to the Party as a whole, where the characterization would be more (but hardly fully) adequate. Such an approach is suggested by various assertions that political outlooks, such as the "rural orientation," came not so much from the CCP's top leadership but from the mass membership. For example, Lee argues that "the root of the Maoist ultraleftist tendency should be traced back to the cadre corps. . . . In this sense Mao's peasant mentality represented rather than shaped those of the majority of cadres" (pp. 73–74). But given all we know about the leader-dominated Chinese system, the suggestion that fundamental regime policies largely *reflected* the social characteristics of ordinary CCP members, which were in fact quite different from those of the top leadership, is very hard to credit.

There is, however, more to credit the notion, also advanced by Lee, that much political conflict can be explained by the concept of "situational groups," that is, groups forged by the impact of particular state policies on their members. Here he usefully adopts the well-established distinction among the victims, survivors, and beneficiaries of the Cultural Revolution in explaining developments in both the last days of the Maoist period and the early post-Mao era. But apart from some errors of fact (e.g., the treatment of Chen Yun, perhaps *the* major architect of the reform of the cadre system, as essentially having little to do with it), shortcomings to the approach are reflected in the contradictions within Lee's account. Lee astutely ascribes the impetus for reform to the rehabilitated victims of the Cultural Revolution, who, having suffered Maoist excesses and had the opportunity to reflect upon the shortcomings of the system during their period in the wilderness, became "born-again reformers" once returned to power (p. 168). Yet key members of this group, including Chen Yun, Peng Zhen, and Bo Yibo are elsewhere pictured as conservative opponents of reform (see, e.g., pp. 285–86). While there undoubtedly is something in this, too, the book lacks a systematic analysis of the paradox. It offers neither a careful empirical examination of the politics of the "old revolutionaries" nor a theoretical explanation of their presumed shift of position.

Finally, the discussion of the background of the new generation of Party technocrats provides some rich detail on their well-known characteristics but leaves unsettled major questions concerning the politics of this generation. Indeed, quite sensibly Lee cautiously canvasses alternative theories concerning the tendencies of technocratic leaders and, on more than one occasion, notes that no definitive conclusion is possible. Nevertheless, he does have a preferred view that the technocratic concern with results, rather than ideology, will result in a greater tendency to bargain and compromise, the likelihood of a collective style of leadership, and fewer power struggles and purges than marked the rule of the old revolutionaries (see p. 407). Still, as Lee acknowledges, the relative lack of close personal ties among the technocrats could result in division or stalemate; and a detailed examination of elite politics during the reform era (only spasmodically undertaken in the book) might show the restraining hand of the surviving old revolutionaries on their younger colleagues, who would possibly have been much more prone to open conflict if left to their own devices. Most of all, a careful reconstruction of reform politics would arguably show that the likelihood of polarized, as opposed to consensual, politics has more to do with the problems and stresses facing the leadership than with the characteristics of particular elite groups.

University of Sydney Frederick C. Teiwes

Self-Interest and Public Interest in Western Politics. By Leif Lewin. New York: Oxford University Press, 1991. 146p. $48.00 cloth, $16.95 paper.

Leif Lewin sets for himself a very imposing task with the first line of this book: "Is it self-interest or public interest that predominates in Western democracies?" It is Rousseau's question, Are people guided by their individual wills (democracy being but the sum of these individuals wills) or by some general will that is distinct (or at least distinguishable) from individual will? This is a question that has concerned political scientists of all stripes and colors, Lewin argues; but because they make different assumptions and work at different levels of abstraction, they are often talking past one another, rather than to each other. Thus, the field as a whole has reached no consensus on how best to structure government to foster the common good. The aim of this book is not to add to the debate in any one of the areas of political science that have addressed the self-interest/public interest question but, rather, to encourage cross-fertilization between the different schools of thought.

To achieve this goal, Lewin begins by sketching the nature of the debate in the rational choice, political philosophy, and electoral behavior subfields. Following Bentham, Condorcet, Hobbes, Mill, and Smith, the "public choice" rationalists assume that people are guided by their self-interest, while the "social choice" rationalists ask whether what is best for society is but the sum of what is best for its individual members. Philosophers concerned with this problem (Lewin relies mostly on Sen and Harsanyi) doubt that anyone can know what is in a person's self-interest (even that person) or, consequently, debate how best to determine what is the general interest. Electoral researchers have explored whether the general public lives up to the classic democratic ideals of the rational, informed voter guided by a disinterested view of the common good, as opposed to being the irrational, unthinking, socially determined or passion-driven voter that Madison feared.

The bulk of the book, however, is devoted to a review of the evidence for the basic self-interest assumption of the public choice school. We hear from Downs (*Economic Theory of Democracy*, 1957 and *Inside Bureaucracies*, 1967), Buchanan and Tullock (*Calculus of Consent*, 1962), and Niskansen (*Bureaucracy and Representative Government*, 1971) that voters are narrowly self-interested; that parties and politicians are spineless, programless vote maximizers; and that bureaucrats care only about increasing their own budgets. These classics of the public choice literature structure the remainder of the book, as Lewin explores whether voters, politicians, and bureaucrats are as self-interested in their actions as the rational choice literature says they are.

The evidence turns out to be a devastating attack on the self-interest assumption. The public rarely vote their own pocketbooks; there is little or no evidence that politicians try to create "electoral business cycles" or that parties feel free to adopt any position they believe the majority of the public favors. Moreover (while the evidence is much less complete here), there is also little or no support for the assumption that bureaucrats are budget-maximizers.

The strengths of this fine, concise book are the diversity of areas covered and the author's awareness of the North American, European, and Japanese literatures on all of these topics. I would strongly recommend it to the participants in any of these debates. As a scientist, I appreciate Lewin's insistence on *evidence* as he attempts to adjudicate between the advocates of the self-interested and public-interested views. Paradoxically, the major limitation of this book is that same focus on evidence, or (more precisely) the limited nature of the available data. Although I cannot claim to be an unbiased observer of this debate, to my mind, the evidence against self-interest as a major motivation for political action is overwhelming.

The case for the public interest alternative is not nearly as strong, however. To begin with, we are shortchanged on any discussion of the philosophical underpinnings of the public interest viewpoint. Rousseau and Locke are not seriously mentioned until the end of the final chapter. And, as Lewin acknowledges, the evidence for some sort of public interest motivation is generally lacking—limited primarily to voting behavior research. This is not a criticism of the book so much as a criticism of those of us practitioners who develop the theories and provide the evidence to address this question. The fact still remains, however, that if we can reject the hypothesis that self-interest guides most political activity in Western democracies, we, as a field, are nowhere near being able adequately to test the alternative hypothesis that public interest is a major motivator of that same behavior.

Rutgers University RICHARD R. LAU

The Religious Challenge to the State. Edited by Matthew C. Moen and Lowell S. Gustafson. Philadelphia: Temple University Press, 1991. 320p. $44.95.

This volume is the product of a conference on religion and politics held at the University of Maine in 1989. Although the title accurately describes the general thematic focus of the collection, many approaches to various aspects of the problem are represented in the essays. The editors' introductory essay provides an abbreviated historical contextual background to the problem and thumbnail summaries of the individual essays. However, it provides neither a conceptual framework within which these essays could be integrated nor an analytical elaboration of their theoretical significance.

Although the editors state in the preface that the book is primarily intended for scholars, a number of shortcomings diminish this collection's significance as a scholarly work. Only the essay by Matthew C. Moen appears to be based on original research using primary data (in this case, interviews, documents and publications of the Christian Right in the United States). The others are either primarily or exclusively based on the analysis of secondary sources (mostly the work of other scholars). Unfortunately, few of the contributors bring new perspectives through the application of different theoretical perspectives or original analytical insights to their data. The editors state, "Because the approach of the contributors is normative and historical, the volume is accessible to a wide readership" (p. x). The descriptive nature of many of the essays diminish their scholarly value.

No contributor refers to any of the other contributions in the volume. This is particularly unfortunate because it wastes an opportunity for more comparative analyses and the potential to refine conceptual frameworks. For example, the suggestive insights of Damian Fernandez regarding the role of popular and political religion in the Cuban Revolution could have profitably referred to Allan Metz's discussion of civil religion in Israel. Fernandez asserts that political religion provided a source of legitimacy for the Cuban Revolution but fails adequately to demonstrate how this was done. To paraphrase Clifford Geertz (quoted by Fernandez), he fails to show how the symbols symbolized. The use, in reference to religion, of terms like *popular*, *political*, and *civil* without conceptually defining and refining them is a missed opportunity of the whole volume.

Metz (a specialist on Latin America) attempts to cover too much ground in his survey of the highly complex nature of religion and politics in Israel. Borrowing heavily on the pioneering work of Charles Liebman and Eliezer Don-Yehiya, he patches together what he terms a "theoretical framework." Unfortunately, this framework is no more than a few generalizations about the nature of the relationship between religion and the state. The work of Liebman and Don-Yehiya is important for the originality of their primary research but not for its theoretical sophistication. They use Robert Bellah's notion of civil religion as a focus for their most important book, a concept Metz does not even utilize in his analysis. His two case studies add no new insights, either conceptual or substantive, to this rather unfocused and unoriginal essay.

Another even more glaring example of the lack of communication between the contributors is the fact that neither Ann Lesch, who writes knowledgeably about the Muslim Brotherhood in Egypt, nor John Voll, who examines the same subject as one of the four cases of Islamic fundamentalism in Africa, refer to each other's analysis. Voll's case analysis is much more successful than Metz's.

Metz introduces the notion of corporatism in his essay on church–state relations in Mexico but fails to utilize it as a conceptual framework for his analysis and does not even mention the term in his conclusions. Nor does he refer to Gustafson's use of corporatism in his analysis of church and state in Argentina, a compliment Gustafson returns by ignoring Metz's essay. Since Gustafson is one of the editors, his omission is even more glaring. Aside from the introductory essay by the editors, only James Warhola, in his lucid and interesting discussion of religion and modernization in Gorbachev's Soviet Union, makes a single, general reference to the volume as a whole.

A very brief essay by Donald Bain, an expert on strategic studies, on the papal challenge to Soviet regimes in Eastern Europe is most noteworthy for the memorable line, "When the pope speaks, people listen" (p. 138). Mark Bartholomew's short analysis of the relationships between the church, the peace movement, and the Social Democratic party of Germany is both knowledgeable and informative.

The volume lacks both an index and a general reference list. Both omissions undermine the utility of the volume for scholars. It appears as if this volume was rushed to press before rapidly changing events could make the book appear outdated. The editors would have done better to have encouraged the contributors to refine their essays and to refer to one another's contributions and used that extra time to write a concluding essay that would bring out the general implications of the findings of the individual authors. Unfortunately, in its present form, this volume makes a very modest contribution to the scholarly literature.

Rutgers University MYRON J. ARONOFF

Political Participation and Democracy in Britain. By Geraint Parry, George Moyser, and Neil Day. New York: Cambridge University Press, 1992. 509p. $84.95 cloth, $34.95 paper.

British specialists are not the only likely readers of this impressive research work. It is a major contribution to our discipline's understanding of political participation in democratic societies generally. The large array of data is British, but the authors present and analyze their findings in an intellectual context that is hardly parochial and often explicitly comparative. Along with the exploration of hypotheses of their own, they test many findings from earlier scholarly studies of political participation in several nations, referring most frequently to *Participation in America* by Verba and Nie (1972).

The book's data are from a substantial project that Parry and Moyser directed, as principal investigators, with funding from Britain's Economic and Social Research Council. Research, in 1984 and 1985, involved not only a national sample survey of nearly sixteen hundred people but also a survey of about the same number in six diverse localities, plus interviews with three hundred selected leaders in those same localities. Questions were designed to learn the extent and nature of political participation, usefully categorized as voting; party campaigning; collective action (through meetings, petitions, and other group pressures); contacting public officials; and direct action (marches, blocking traffic, political strikes, and boycotts). Attention concentrates on the minority of citizens whose participation goes beyond voting. Rightly, as the authors say, that kind of British participation has been much less fully studied than British voting behavior. They fill a large portion of the gap in our scholarly knowledge by telling us about the numbers active in each category, the social status of these participants, the resources or other factors that help account for participation, the impact of participation on policy outcomes, and the satisfaction that participants derive from their activities.

So general a description does not do justice to the wealth of information and analysis that the authors provide. An idea of the book's richness, as well as of its quantitative character, may be inferred from the presence of 49 figures, 7 charts and maps, and 111 tables. The expository prose is ample and clear. Moreover, the authors put their study in a coherent theoretical, even philosophical, framework, reflecting a concern with the significance of participation in a democratic society. Especially enlightening is the discussion of the strongly local emphasis of most participant activities. It was first discovered in the national survey and then confirmed and illuminated in the several local studies that activists are more often motivated to influence policies close to home than they are to take up the broad issues that command the attention of the national news media. Local, not central, government is the usual target. Issues beyond the everyday concerns of citizens are to a larger extent left to officeholding representatives between elections. The authors evaluate the consequences for stable and effective democratic government with great sophistication, taking into account the contrasting traditions, on the one side linked to Rousseau and on the other, to Schumpeter.

Citing a few of the book's many other findings will illustrate its significant lines of inquiry. Americans, accustomed to unfavorable comparisons of voting turnouts in the United States with those in Britain and other nations, will be interested in data showing British participation in certain political activities to be at a somewhat lower level than American participation in similar categories. A more generally intriguing matter is the special place that party campaigning occupies in the responses to questions about whether the various participatory activities led to increased knowledge. Whereas most activities were not seen by participant–respondents to have added to their knowledge, the major exceptions were two forms of party campaigning. Canvassing for votes and behind-the-scenes party clerical work were reported as educative in their effect. But the absence of such learning in nonparty political activities did not mean that the relevant participants failed to find satisfaction in their experience. A high degree of satisfaction characterized most participants. Again, the local surveys fully confirmed the national survey. "Participants," the authors conclude, "displayed remarkable degrees of contentment with the outcomes of their dealings with local authorities" (p. 403). Their sense of individual efficacy was at a respectable level, even for some who expressed considerable political cynicism. Here, as elsewhere, the relationship is complex. Indeed, the complexity, along with multidimensionality, is a principal theme of the authors' careful analytical study.

University of Wisconsin, Madison LEON D. EPSTEIN

The Apartheid State in Crisis: Political Transformation in South Africa, 1975–1990. By Robert M. Price. New York: Oxford University Press, 1991. 309p. $55.00 cloth, $16.95 paper.

In this study of recent South African politics, Robert Price distinguishes processes of transition (changes in a society's formal political order, consisting of rules and institutions) from processes of transformation (changes in a society's substructure of domination, consisting of social interactions, cultural norms, economic activities, and informal power relationships). Price argues that changes in the latter make possible change in the former. In the case of South Africa, a two-decades-long process of economic and social transformation have led to the current talks on changing the formal rules that govern South African politics.

Price observes that apartheid was established in the aftermath of World War II in order to bring Afrikaners up to socioeconomic parity with South Africans of British descent, to reverse gains made by blacks in South Africa during the war years, and to respond to an international system slowly developing norms of decolonization. While never accomplishing this last goal, by the early 1970s, apartheid had transformed the interests and demographics of Afrikaners to the point where their elites saw apartheid as a drag on the economic growth needed to preserve white domination.

The imperative of reforming apartheid emerged during a period of regional confrontation. The collapse of Portuguese colonialism in Southern Africa created the threat of a region unified against white domination in South Africa. The success of liberation movements in Angola and Mozambique spurred on such movements in Zimbabwe, which in turn emboldened black protest in South Africa itself. A newly found confidence among South African blacks, combined with the economic deprivations of apartheid, created the environment in which the Soweto uprising of 1976 took place. The regime responded to the Soweto riots with brute force and repression, which prompted the international community to increase pressure on South Africa to change. The pressure—economic and cultural isolation—exacerbated the downward economic spin of South Africa and pushed the regime to undertake reform. Thus began a slow, roundabout effort to eliminate apartheid in order to maintain white power and privilege.

Price, drawing from the insights of Tocqueville, describes how the reforms had unanticipated consequences that strengthened internal opposition to white supremacy. Renewed opposition was met with renewed repression; renewed repression was met with new international condemnation and stiffer sanctions; renewed international pressure increased the need for reform and strengthened domestic opposition, which led to more repression. By the mid-1980s a situation that Trotsky had described as "dual power" existed in South Africa. What Price refers to as the "security contradiction" of the white regime could only be broken by the willingness of the regime to negotiate a transition in its formal political rules and constitution.

Price knows South African politics, economics, and society and writes well. He recognizes that developments in South Africa are a product of three levels of analysis—international, regional, and national—which he vividly and impressively weaves together in order to explain how change has come to South Africa.

Weaknesses in Price's analysis come from two sources. First, he tends to see various changes in South Africa as linear and therefore unsusceptible to reversal. For instance, early on, Price states that changes in the substructure of society will necessarily lead to transition, a claim that he subsequently backs away from (pp. 4, 285). Elsewhere in the book, he claims that economic processes irrevocably shattered Afrikaner identity and values (pp. 83–85). Yet if we have learned anything about political identities of cultural groups, it is that they wax and wane and that sentiments of group identity can disappear only to reemerge at a later date.

Second, Price's formulation of the relationship between substructure of domination and structure of formal order suffers from imprecision. Consider, for example, the grab bag that makes up the substructure of domination: social interactions, informal norms, economic activities, and informal power relationships. Should we expect changes in these categories to be synchronous? Do any of the categories take precedence over the others? Here, the two authors that inform Price's conception of society differ. Tocqueville saw revolution as inevitable in France because of the disjuncture between the formal rules of the Old Regime and the informal constitution of French life. In his history of the Russian Revolution, Trotsky saw the key disjuncture as one between the formal political rules that benefitted a particular class and economic changes that had rendered that class obsolete.

In the South African case, there have been important changes in some of the categories of the substructure. But the transformation that Price describes is an incomplete one; changes coexist with important continuities. Two examples come to mind: economic activities that continue to benefit the white population of South Africa disproportionately and mental legacies of racism. If Price's overall thesis, namely, that formal political rules must rest on the informal substructures of domination, is correct, then any transition to majority rule in South Africa will stand on shaky foundations; the formal constitution will proclaim equality, while the informal one will mock such claims.

Even with its faults, Price's book is a stunning accomplishment. It towers over recent analyses of the South African situation and puts to shame recent historical sociological treatments of revolution that amount to histories written to illustrate a theoretical assertion like the autonomy of the state or the nature of class relations between lord and peasant. Price, like Trotsky and Tocqueville, constantly ranges back and forth between historical facts, patterns, and relationships on the one hand and broader theoretical concerns on the other. And like Trotsky and Tocqueville, Price's keen insights come not from some overarching theoretical perspective that by insisting that it explains everything, explains nothing, but from familiarity with historical detail and sociological method.

Johns Hopkins University STEPHEN J. STEDMAN

Democracy and the Market: Political and Economic Reforms in Eastern Europe and Latin America. By Adam Przeworski. New York: Cambridge University Press, 1991. 210p. $39.50 cloth, $12.95 paper.

This book deals with two of the most important and least understood phenomena occurring in the world today: democratization and the transition to more market-oriented economic systems. Przeworski is a wise and insightful analyst unusually well placed to observe the transitions in both Eastern Europe and Latin America closely and thus, perhaps, uniquely qualified to theorize about them. The strength of the book lies in the effort to create general models of processes that are usually treated descriptively.

The stylized political world that underlies these essays is populated "not by freely acting individuals but by collective organizations that are capable of coercing those whose interests they represent," latent groups that might become organized, and individual voters who can periodically reward or punish political leaders but seem not to affect the strategies of the groups whose interactions with each other determine political outcomes (see pp. 12, 39). This is not the only possible stylization of a democratic political system, and it occasionally runs into inconvenient events that require some interpretative surgery to make them fit. But an imperfect model (and all are imperfect) is always better than no model; and this model gives Przeworski the leverage he needs to theorize in areas that have been notorious as analytic swamps.

I shall concentrate on three of the numerous ideas in this wide-ranging book that seem to me to form the logical core: the requirements for stable democracy, the choice of new democratic institutions during transitions, and how the transition to market-based economies affects the probability that stable democracies will arise from the breakdown of authoritarianism.

Przeworski's treatment of stability begins with the observation that democracy survives when groups that lose out in the policy process continue to comply with the democratic rules of the game. He therefore focuses on losers, developing an abstract model of their preferences as a way of exploring the conditions that would affect their willingness to remain in the democratic game. In the model, losers' compliance depends on the stakes of the game, the probability of winning in the future, and their discount rate (all affected by, among other things, the institutional structure of a particular democracy), as well as the probability of successful subversion.

Once it has been established that institutions affect the probability that democracies will consolidate themselves, it becomes important to understand how different concrete institutions affect both policy losers' calculations and how political actors choose new institutions during transitions to democracy. Przeworski says little about the former but devotes considerable attention to the latter. In the Przeworski world, institutions emerge as the result of bargaining among political "forces." Where the "relation of forces" is known and uneven, institutions will reflect the interests of "a particular person, party, or alliance" (pp. 81–82). Where the relation of forces is known and balanced, the outcome is indeterminate. Where the relation of forces is unknown, institutions can be expected to emerge that contain checks and balances, reduce the stakes of competition, and provide guarantees to potential minorities. Anarchic and uncertain transitional situations may thus, counterintuitively, be most conducive to the emergence of stable democracy in the long run.

In the final essay of the book, Przeworski moves from the general issue of democratic consolidation to the particular threat to consolidation posed by concurrent transitions to market-based economics. In this essay, he has many sensible and insightful things to say about the uncertainties and short-term costs of economic transition; and he develops a series of perceptive arguments to explain the choice of radical, over gradualist, adjustment strategies.

A risk-averse person would not have ventured into this subject so early on in the process, when outcomes were still so uncertain. But strong theory gives scholars the courage to get up and do what needs to be done. With the advantage of more experience of reform than was available to Przeworski, however, it now seems that his conclusions about the political consequences of economic reform are too pessimistic.

As Przeworski notes, the costs of economic transition for organized labor are likely to be high. Because he sees the political process as dominated by the struggle for advantage among organized groups, Przeworski concludes that governments will only be able to pursue adjustment through one of two strategies: consultation and negotiation with groups, especially unions, that will be hurt by reforms (concertation), which he sees as unlikely in developing countries for several sound reasons; or else, insulation of policymakers from affected groups, which he sees as undermining democracy. In fact, however, several governments (most notably, Argentina, Turkey, and Thailand) have carried out substantial reforms (without concertation) while maintaining political support through at least one election.

By the clever sequencing of reforms or dramatic reductions in inflation, they have made reforms palatable to the average voter while imposing heavy costs on much of the unionized working class. They have, in other words, found support for themselves and their policies among the relatively unorganized masses of voters while politically marginalizing better-organized groups. Although relatively unimportant in the book as a whole, this seems to me a good example of the limitations of the Przeworski stylization. In democracies, outcomes sometimes depend on interactions between political leaders pursuing their own interests and freely acting individual voters, not organizations "capable of coercing those whose interests they represent."

To return to the whole, this is an important book on important topics. Its vivid imagery, lively language, and unexpected but apt metaphors make it highly enjoyable to read. Like other work by Przeworski, it will help structure the thinking of other scholars. It will also frustrate anyone who attempts to use the arguments in it as the basis for building a sturdier accumulation of knowledge. The empirical referents of concepts central to the models are so underspecified that it is hard to imagine how one would go about testing them. How, for example, could you tell whether the "relation of forces" was balanced or not? How could you tell whether an unknown balance of forces resulted in institutions that limit the stakes of the political game without any information about what concrete political institutions have that effect? Nevertheless, the book is thoughtful, imaginative, and provocative. It will cause its readers—scholars, political activists, graduate students, and even some undergraduates—to think interesting thoughts.

University of California, Los Angeles BARBARA GEDDES

Politics within the State: Elite Bureaucrats and Industrial Policy in Authoritarian Brazil. By Ben Ross Schneider. Pittsburgh: University of Pittsburgh Press, 1992. 337p. $49.50.

This lively and important book is an account of the triumphs and failures at the vanguard of Brazilian policy-making represented by the steel and mining sectors. The coverage is broader than the subtitle indicates; for the analysis ranges over the decades of developmentalism, democratic as well as authoritarian, from the postwar years to the present. One of the implications of Schneider's research is that changes in political regimes do not matter much for the way the Brazilian *sistema*—the deep structure of power—operates.

The paradoxical idea behind the analysis is that the distinctive trajectory of bureaucratic careers in Brazil, dominated by agency-hopping and political, rather than civil service–style, appointment, undercuts institutional predictability at the same time that it permits and even facilitates a creative mix of interorganizational competition and coalition building. Although he does not resort to the jargon, the evidence assembled by Schneider can be understood as documenting the vagaries of the labor market internal to the Brazilian public sector and its consequences for getting the job done.

Politics within the State is worth reading and rereading, first, simply as an insightful phenomenology of the sprawl and folkways of the Brazilian state. The organizational guerrillas who are the subjects of story will have no trouble recognizing themselves and the arcane recesses of their environment. In going through its pages I was reminded of the vulnerable artfulness of Brazilian soccer—almost all offense, given to *o lance genial* (the stroke of genius), only to collapse the next moment—for all one knows, out of boredom with the game. The book is fun to read. Schneider writes suspiciously well.

Schneider also raises important questions about the links between reform-mongering, organizational design, and economic strategy. The challenge involved in dealing intelligibly with bureaucratic politics—and with the Brazilian public sector, in particular—comes not only from the size of the thing itself but also from the simultaneous vastness and inadequacy of the literature on the subject. Like many others before him, Schneider attends to the hard-chargers, the managerial zealots bent on compressing "fifty years of development in five," as a Brazilian president once declared. As Delfim Netto, the *primus inter pares* of the barons of grandeur, put it with still greater hyperbole, "Development is the moral equivalent of war." Schneider himself suggests that this focus ignores the agencies charged with services, the cultivation of human capital, and, more seriously, the larger bureaucratic context (less flashy, more inertial, closer to the backwaters of Brazilian politics) above which the superstars of capital accumulation and futuristic technology move and shake. While attention is given to the coziness between ministerial officials and their counterparts in private industry and banking, the lack of contact between development hotshots and those engaged in mop-up and peripheral chores like health and education—the clubbiness of winners and their Darwinian exclusion of the timid, the luckless, and the inept—seems crucial to understanding the cumulative inequities that defy democratization. In this light, the pathologies of the American entertainment industry would be a better metaphor for Brazilian development than a balletic soccer match. The analogy breaks down not in the magnitude of the profits or the style of the subculture but in the costs to losers.

Schneider's criticism of the assumption that bureaucratic fragmentation regularly leads to inefficiencies is reasonable. Yet his contention that development projects in Brazil succeed more frequently than expected is problematic. If the baseline for this claim is that fragmentation inevitably gets in the way of intended results, then the hypothesis is bound to be correct. However, a more sensible standard for evaluating implementation might start with a statistically normal distribution of outcomes. The idea would be to estimate what organizational formats produce better (or worse) results than they might do randomly. The logistics of testing this null hypothesis (hardly a model but better than no criterion at all) would involve a larger number of cases than the four examined by Schneider, who is careful to list a number of conditions and factors that contribute to success and failure in variably coordinated and chaotic settings. Nevertheless, baldly stating criteria for success and failure or else giving reasons for not doing so would clarify the particular outcomes.

But a significant implication of *Politics within the State* is that using a random walk as a point of departure for understanding policy implementation in Brazil—while more realistic than condemning the process to failure by fragmentation—is historically inaccurate. Over time, and at an accelerating rate over the past decade, the chances of success seem to have deteriorated. This may be the nightmare effect of the administrative turnover that boosted innovation earlier in the public sector. The circulation of officials almost certainly created incentives for each of them to embellish his curriculum vitae by launching new ventures without incentives to complete the projects during their brief tenures. Such a proliferation of commitments helps explain how aggregate bottlenecks and irreversible paths build up from tactical improvization. It also lends a certain pathos to the cycle of enthusiasm and exhaustion—the manic-depressive syndrome—common among the talented individuals responsible for the fortunes of the Brazilian state.

Adept at cutting deals and finding shortcuts, energetic in resolving problems on the spot, Brazilian bureaucrats and politicians have seen their institutions veer out of control and their developmental visions thwarted. Next to the distinctive combination of patrimonialism and piecemeal empiricism, the anomaly of vaunting ambition and shortsightedness may be as close to a fixture of Brazilian political culture as one is likely to encounter in so fluid a spectacle. Expedience as an ideology is not pragmatism. This ethos emerged in the ramshackle constitution of the New Republic; and it produces the sort of scenario that periodically sets the military tapping its fingers at yet another demonstration that democratic politics may be, if not quite catastrophic, less than the sum of its parts.

If development in Brazil has become a race against worsening odds, for reasons having to do both with institutional perversities and structural distortions embodied in the concentration of wealth, it may be instructive to set the Brazilian impasse against the less-discouraging experience of countries like Spain and South Korea, with comparable histories of authoritarian and democratic episodes—not to mention cases closer to home, such as Mexico and Argentina. In conveying the drama and the stakes of Brazilian politics and in offering a provisional interpretation, *Politics within the State* brings us closer to placing the terrible enigma of Brazil in comparative perspective.

Arizona State University PETER MCDONOUGH

Disorder under Heaven: Collective Violence in the Ming Dynasty. By James W. Tong. Stanford: Stanford University Press, 1991. 325p. $39.50.

In this book James Tong offers a quantitative, diachronic, rational choice, state-centric study of collective violence during the Ming Dynasty. Quantitatively, he marshals data on 630 violent events and a variety of data on the economic and political context surrounding them. Temporally, his study spans the 277 years of the Ming era. His approach is rational choice in that he hypothesizes that the decision to join in collective violence was the product of individual calculations of the odds of survival in time of dearth and hardship and the odds of survival as an outlaw in the face of government repression. His approach is statist in that most of his measures of the odds of surviving either hardship (e.g., provision of government relief) or as an outlaw (e.g., proximity of government forces) are measures of state performance and capability.

Tong's study is exemplarily straightforward and parsimonious. In an introductory chapter he poses his research tasks, namely, to describe and explain the major forms of collective violence during the era; sketches out his model; and describes the major patterns visible in his data, namely, a preponderance of collective violence in the South, in the latter half of the dynasty, in rugged and remote regions, and in the form of banditry. Chapter 2 elaborates the model—a choice model based on the interplay of individual motives (primarily survival), the resources available to the people, and the opportunities for contention open to them under the political status quo—and the data, with terms defined and operationalized using the county–year (of which there are 303,869, given 277 years and 1,097 counties) as the unit of analysis.

Chapters 3 and 4 are the centerpiece of the book. Chapter 3 presents in descriptive detail the different types of collective violence: and in chapter 4 two scales (composites of four measures of survivability and six measures of survivability-as-an-outlaw) are constructed and posed against the collective violence data for each unit of analysis. Contention across county–years varied directly with survivability-as-an-outlaw and inversely with the survivability of hardship; together, they powerfully indicate that (as hypothesized) banditry as a response to hardship was by far most likely in regions and at times in which the survivability of hardship was lowest *and* survivability-as-an-outlaw was highest.

Chapter 5 goes into detail as to why the observed relationship should hold, focusing primarily on regime capabilities and how they shaped the opportunity structure of collective violence both in toto and as they varied across time and space. Chapters 6 and 7 consider two alternative explanations of collective violence: that it results from rapid social and/or economic change and that class conflict is the force behind it. The data from Ming China provide little support for either. Motives, resources, and opportunities appear to be unquestionably a superior explanatory constellation.

The special strengths of this book are its parsimony, persuasiveness, and comprehensiveness. Without falling into simplistic number crunching, Tong presents his model and his data thoughtfully and with appropriate caveats, uses data both illustratively and analytically, and winds up his analysis (including relevant historical background and consideration of the two alternative models) in only 214 pages of text. Moreover (perhaps because it is so simple and straightforward), his argument is persuasive, at least to me. The components of Tong's scales seem to be valid indicators of the concepts in question, and his operational measures are valid measures of the component variables. Finally, Tong's study is comprehensive. The genre of historical political sociology includes few studies of collective violence covering any society so intensively over such a long period. Although equivalent, the quantitative analysis in Charles Tilly's *Contentious French* is neither so systematic nor so permeating. In Tong's words, "For no other premodern system are annual, local-level data on collective violence as available as they are for the Ming" (p. 9); and, as he points out in his conclusion, data collection efforts presently underway in China hold out the promise of extending this study by several centuries in both directions from the Ming.

This said, I still have three reservations regarding this study: a worry, a caveat, and a disagreement. The worry is that there may be more data-underreporting than Tong argues. A similar data set for a similar period

(1590–1877) for another preindustrial society (Tokugawa Japan, with one-fifth the population of Ming China) includes 7,664 incidents of collective contention, of which 1,258 involved aggressive or violent action and 811, outright violence. By any criterion of comparison, China seems extraordinarily quiescent.

The caveat is that this study is not quite comparable to many of those (by, e.g., Tilly, Gurr, and Hibbs) that come quickly to mind, because the collective violence it focuses on is overwhelmingly *collective, armed crime,* not just political conflict or protest or violence, or social contention more broadly construed. Thus, the model is more likely to fit Jesse James than Emiliano Zapata. Tong gives good reasons for this focus, but it may still restrict the comparability of the study.

Finally, I disagree with some of the assertions explicated in Tong's model. In the first place, I do not agree that "in premodern China, as in other agricultural societies, subsistence was the central objective of the peasantry." (p. 78), unless *subsistence* is defined to include *status* subsistence, that is, keeping one's family and community economically, politically, and socially viable. I think that the sorts of economistic calculating studied in Samuel Popkin's *Rational Peasant* were always at work and could lead to collective violence of various types. Nor do I agree with the corollary that free-riding was not a problem in collective violence because participants sought only private goods and that "the desire to seek public goods" is not a "plausible explanation of motivation for premodern collective violence" (p. 199). It *is* true for *this type* of collective violence—banditry—but not for the sorts of political and social violence studied by others, in which public goods could play a powerful role, as the example of Zapata suggests.

In other words, this is not a disagreement with what Tong has found in his data but a disagreement with the generality of his findings. Certainly, on its own terms, Tong's study is solid, persuasive, and informative; and it deserves, and rewards, reading by anyone in the field of contentious collective behavior.

University of North Carolina James W. White

Race to the Swift: State and Finance in Korean Industrialization. Jung-en Woo. New York: Columbia University Press, 1991. 280p. $35.00.

It is increasingly clear that the role of finance in the extraordinary growth and structural transformation of the East Asian economies is a crucial topic. Nowhere was financial policy used more aggressively than in Korea. It is also clear that the preferential allocation of capital played the crucial role in the emergence of Korea's giant business groups, or *chaebol.*

Jung-en Woo construes finance broadly and approaches it historically. She locates the origins of Korea's strong state primarily in foreign influences, particularly the security concerns of the United States, and the regional economic concerns of Japan as mediated by the United States. Thus, her approach builds on dependency theory but is more determined by global security politics than the logic of international capitalism and allows for considerable room for maneuvering on the part of the peripheral state. A second major theme is the creation of a capitalist class and of "Korea, Inc" through a credit-based financial system. Subsidiary arguments include the repeated subversion of orthodox financial and trade reforms urged on Korea by the United States, the International Monetary Fund, and the World Bank and, in contrast, successful economic intervention by the Korean government.

An initial theoretical chapter is followed by a discussion of colonial Korea. The experience of Japanese colonialism, including precocious development of an activist financial system led first by the colonial state and then by the Japanese *zaibatsu* (but never by local entrepreneurs), became a template for postwar development, she argues. Import substitution and aid grubbing under Syngman Rhee in the 1950s were not simply corrupt and ineffective but laid an economic base for the 1960s and delayed domination by Japan. Takeoff in the 1960s was made possible in good part by Japanese investment and support for the ruling party. High-interest-rate policies adopted under American pressure were incomplete, and led to a huge foreign debt, since interest rates abroad were lower; debt, in turn, led to instability and authoritarianism and, finally, a return to low real interest rates and bureaucratic allocation of capital.

Chapter 5 covers the "Big Push" toward industrial deepening of the 1970s (which the author sees as motivated by the Nixon doctrine and global economic shocks) and provides thumbnail sketches of development in five key industries. Chapter 6 is the heart of the book: "A credit-based financial system, mediated by an interventionist authoritarian state, became the basis of Korea Inc." (p. 149). The state shoveled money to exporters and heavy industry despite their lower profitability and used control over access to capital to ensure economic performance. The state socialized the risk of the *chaebol,* while small firms and households were forced to rely on the unregulated curb market. By the 1980s, she argues, changes in the security environment and global economy and the skyrocketing of nonperforming loans convinced Korean technocrats, now overwhelmingly "transnational elites" trained in the United States (p. 190), to liberalize the financial system; while by no means complete, this liberalization and the growth of the stock market and of nonbank financial intermediaries has marked a decisive change in Korea, Inc.

The strength of the book is clearly in history, particularly on the influence of the Japanese period (a controversial issue) as a model. The in-depth treatment of American and Japanese policies toward Korea is unusually well weighted. The discussion of the implications of regional economic integration for Korea is interesting and ironic, in light of recent developments: dual hegemony in Asia with a predominant economic role for Japan is not new and was, in fact, designed by the United States (pp. 88–92, 184–87).

The book does suffer from some important omissions. It provides no sustained account of politics, particularly for the period since the fall of Rhee. There is surprisingly little analysis of the actual details of financial policy and virtually none on financial and other bureaucratic institutions (see pp. 84, 103, 159). The Ministry of Finance manages only 3 citations in the index, while Nixon receives 15. Similarly, little attention is paid to the actual organization of the *chaebol;* and the unusual definition conflates group structure and market power (p. 149). Moreover, while Woo concludes that the flip side of the development state was a "racketeering state," questions about rent seeking are dismissed in two sentences (pp. 69, 200).

Looking comparatively, it is impossible to explain from Woo's externally dominated account why Taiwan's financial system was rigidly constrained, rather than mirroring Korea's, and why high real interest rates there led to increased domestic savings and very little foreign debt. Similarly, despite her characterization of Korea as a "clone" of Japan (p. 176), the *chaebol* operate very differently from Japan's *keiretsu*. The Korean government dominated businesses and, on occasion, compelled them to merge in ways that have no precedent in Japan. The lack of a concluding chapter also weakens the analytic and comparative aspects of the book.

Finally, while the bibliography is extensive, the author implies that no one has ever recognized the importance of finance in Korea before; and that is simply not true. Important recent works by Alice Amsden, Tun-jen Cheng, and others receive at most glancing recognition. Had the publication of *Race to the Swift* not been delayed, its arguments would have been more novel, and its emphasis on dependence, less dated. It is best seen as a broad introduction to the history and international context of Korean industrialization, rather than as a cutting-edge analysis of Korean finance.

University of California, Berkeley GREGORY W. NOBLE

INTERNATIONAL RELATIONS

The World Trading System at Risk. By Jagdish Bhagwati. Princeton: Princeton University Press, 1991. 156p. $16.95.

Losing Time: The Industrial Policy Debate. By Otis L. Graham, Jr. Cambridge: Harvard University Press, 1992. 370p. $29.95.

Parallel Politics: Economic Policymaking in Japan and the United States. Edited by Samuel Kernell. Washington: Brookings Institution, 1991. 390p. $36.95 cloth, $15.95 paper.

The grouping of these three volumes reflects the interconnections between the politics of the domestic economy and the international politics of trade and security. *Parallel Politics* is a valuable collection of Japanese and American essays comparing the two nations' political management of the domestic economy. Because the essays make good reading assignments that present a clear position and sketch the historical and institutional texture of the two cases, the book will be useful in graduate and undergraduate classes.

The American essays tells a consistent and convincing story. "Political Institutions and the American Economy" by John E. Chubb and Paul E. Peterson is an excellent overview that traces the historical development of the American system of political economy. It examines how the collision of new issues and established institutions has stymied the management of the U.S. economy. Historically, the federal government found mechanisms to contain their rivalries while still providing coherent leadership; but those mechanisms have now failed. The presidential/congressional struggle now overlaps the party struggles and blocks effective policy. Matthew McCubbins reinforces this message, arguing effectively in "Party Politics, Divided Government, and Budget Deficits" that the enormous American national budget deficits are the product neither of the Reagan policies nor of congressional abdications. Rather, they are consequences of a divided government that has stalemated. Allen Schick argues in "The Surprising Enactment of Tax Reform in the United States" that astute political management permitted sharp changes in the tax structure, which suggests that sharp changes in policy are possible. If so, then a crucial question falls in between chapters: Why could we find new directions in tax policy but not effectively address the budget problem of a gap between spending and revenue? It would have been helpful if one of the authors had addressed what was different about these two problems.

It is in the Japanese case that we find controversy. Both popular discussion and much academic literature emphasize the capacities of a directive Japanese developmental state to provide industrial leadership. Consequently, a balancing emphasis on local politics is very valuable. The controversial assertion woven into Samuel Kernell's overview essay is that in Japan, appearances and earlier literature to the contrary, politicians, not bureaucrats, run the country. Do we require a revision or a marginal corrective in our understanding of Japan?

This volume provides several competing answers to the question of politicians, party, and bureaucrats in Japan. The conventional view is that there is a substantial difference between the U.S. and Japan in the policy priorities and capacities for direction and policy implementation. Haruo Shimada's essay, "Structural Policies in Japan" supports this traditional view of bureaucratic authority and directed strategies. Michio Muramatsu and Masaru Mabuchi present a story, in "Introducing a New Tax in Japan," in which the single ruling party provides—or comes over time to provide—the capacity to shape policy directions. It is very helpful to our understanding of how the Liberal Democratic party (LDP) influences policy and the capacity for governance. Yukio Nguchi's discussion of budget making is certainly consistent with the increased power of the LDP in conflicts within ministries. But critically, the Ministry of Finance remains the principal player in settling conflicts between ministries. Thus, in this essay, the power is balanced between bureaucracy and party, each having a separate domain. However, Eisuke Sakakiabara's overview of the Japanese political economy, "The Japanese Politico–Economic System and the Public Sector," presents a Japan in which the politicians whose centrally local concerns are grouped in policy cliques and political factions dominate. Neither the Ministry of Finance nor the dominant party, the LDP, play much of a role. Indeed, in his vision, the government stands aside from the market and often competes with it. The worlds of the development state (Chalmers Johnson), policy compacts (Richard Samuels), and the semicorporatist doorkeeper (T. J. Pempel) are no where to be found. This debate is neither resolved nor, really, confronted in the book; and the notion that developmental policy is simply the other face of the "politics of particularism" simply sidesteps the problem and buries the most interesting questions. For example, "collusive solutions to market dilemmas" in most countries simply retard growth. In Japan such arrangements are often elements of "controlled competition" (Murakami and Yammamura, Tyson and Zysman). True, as Sakakiabara argues, the (public) postal savings banks and the private banks compete; but the overall pattern of financial flows is one that has both sustained industrial development and created a particular pattern of competition. Are these financial flows and patterns accidental or created?

The most effective section of the volume is the comparison of structural policy in the United States and Japan. Roger Noll and Haruo Shimada present a common essay that develops a set of premises and hypotheses that guide their individual analyses. They argue together that structural policy is easier to formulate in Japan and more likely to be long-term, even though both countries will have difficulty scaling down inefficient sectors. Noll's lucid essay argues that the United States does not have a coherent and directed policy; nonetheless, he manages to outline the pattern of policy that does influence the industrial structure. Noll's overall argument is not new, but he presents it exceptionally well. The specific discussions of individual policy domains are quite rich and very interesting. I regret, for example, that the section on deregulation could not have been a chapter in itself, so that the supposed success in transportation, energy, and telecommunications in the

1980s could be set more fully against the overt failures in finance and the new problems in telecommunications and transportation. Shimada's essay usefully focuses on the Japanese industrial sector policies that have attracted American attention because of their international dimensions. Importantly, he also considers the Japanese effort to set right distortions in the economy, with primarily domestic consequences—a list that includes the gap between the high nominal income and low standards of living, increased wealth differentials in terms of real estate and financial assets, and differential allocation of economic resources between Tokyo and other regions (p. 285). In sharp contrast to Sakakiabara's presentation, Shimada asserts the critical and coherent role of the state in industrial development. Shimada is not set the task of explaining how a system made up of particularistic politicians can produce a coherent outcome. But the volume, already very good, would have been even better had this question and the difference in views between Shimada and Sakakiabara been exploited.

The editor, Samuel Kernell, proposes as his integrating theme that politics in both countries is centrally about local politicians satisfying the demands of constituencies, particularly those who have suffered in the marketplace. Local politics is certainly aggregated differently in each country because of differences in bureaucracy and party. Nonetheless, the common feature, claims Kernell, is that "rather than implementing some comprehensive strategy of growth, [policy in each country] offers protective cover to ailing and favored sectors of the economy through trade protection, sympathetic commercial regulation, and direct subsidy" (p. 328). Kernell provides an interesting and helpful analytic framework for examining the place of local politics and the mechanisms of political aggregation in the two countries. Kernell's position is not "tested" by the essays but, rather, is an interpretation of, and approach to, the material discussed. One problem with Kernell's approach is that the two particularisms, Japanese and American, produce quite different economic results. A complementary theme, not an alternate one, would be to identify the distinct policy results and ask why the common particularisms of local politics produce such distinct outcomes.

Losing Time, by Otis Graham, is a contemporary history of the industrial policy debate. In a sense, it depicts the story of the economic option that was not adopted in the 1980s, an alternate path to that chosen by the successive Republican administrations. It traces the intellectual and political debate. The detail will be a useful resource for those constructing the story of the politics of American economic decline. Although a carefully constructed history, the book would have benefited from an analytic framework that set the contemporary story in a historical context and provided an interpretation of the domestic politics of the period and an independent interpretation of the competitive and trade problem.

By contrast, Jagdish Bhagwati's *World Trading System at Risk* is a sharply focused defense of the multilateral trading system organized around the General Agreement on Tariffs and Trade (GATT). It is articulate, well crafted, and very frustrating. Bhagwati has an important case to make; and at the core, it is one I would largely agree with. The multilateral trading system has made great contributions to our economic welfare and is now at risk from demands for protection and strategies for advantage. A number of his proposals to reform the GATT system are useful and interesting places to begin discussion. The difficulty is that he dismisses outright the concerns underlying the demands on governments to protect or promote in ways that might threaten the GATT. Those concerns emerge from a vision of trade and an understanding of the terms and consequences of competition that differ from his. The form and language of his arguments preclude debate about the dynamics of trade, what the exact risks to (not just sources of strain on) the trading system are, or what solutions might be appropriate. He presents his arguments as if they were theoretically, analytically, and empirically dispositive. Those who would disagree with his characterization of the problems or seek other lines of solution are, in his view, apostates, holders of false beliefs. Bhagwati argues that "facile views of Gatt's impotence" are given fuel by "ignorant prattle by academic economists . . . unburdened by historical knowledge, scientific expertise, and the sustained scholarly reflect[ion] that these matters require." The list of those criticized directly in the book for various sins including theoretical fallacies (and presumably therefore spewing such ignorant prattle) include such journalists as Robert Kuttner and Jim Fallows, as well as political scientists. The economic credentials of the political scientists and journalists may be easily challenged. But also on the list are such outstanding economists as his Massachusetts Institute of Technology colleagues R. Dornbusch and Paul Krugman; Harvard economist (presently vice-president for economics at the World Bank) Lawrence Sommers; and my colleague, Laura Tyson. Many of us would share his basic concerns. His analysis and his policy prescriptions would gain greater weight by truly engaging the issues, rather than disposing of them with rhetoric. Some examples are helpful.

Consider the place of Japan in the trading system. Is Japan—the world's second largest economy with advanced and sophisticated technology and some dominant industries—continuing to follow developmental strategies that insulate its market from foreigners and promote domestic firms to create advantage in global markets? If so, competitor governments will attempt to defend their firms and competitive position with protection. The GATT will be strained. But in Bhagwati's eyes Japan either is an open, liberal economy or at least is distinct in ways that are not crucial to the trade debate. Consequently, the problem for Bhagwati are Japan-bashers, not Japan. Maybe he is right and Japan is fundamentally an open liberal economy, or at least an economy whose distinct domestic approaches are not crucial to trade issues. But the notion that Japan's protected markets were crucial to its success in the past and remain important to creating advantage and managing declining sectors is not so easily dismissed. Certainly, the debate about nontariff barriers makes argument and evidence complex. Perhaps the allegations are as hard to disprove as they are easy to make, but that does not make them wrong. Bhagwati's Japan is close to that of Sakakiabara in *Parallel Politics*. Shimada's Japan in the same volume fits the model of the Japan-bashers. Arguments by Murakami and Yammamura that Japanese business structure and policy drive a pattern of excess competition, domestic market saturation, and resultant policy intervention and dumping are never confronted and are hardly the fantasies of American Japan-bashers. The technical debate over econometric studies of Japan's trade, some of which suggest not only discrimination but a very unique pattern of trade, is swept aside by the assertion that "the best crafted studies" favor the view that Japan's trade patterns are not distinct. Perhaps; but others, such as Bela Belassa and Edward Lincoln (former director of the Japan Economic Institute and hardly a first-line basher) would disagree about substance and methodology. They offer very convincing alternate interpretations of the data that Bhagwati introduces. There are strong and legitimate alternate positions, and Bhagwati cannot simply wave them away.

Or consider the debate on strategic trade theory. Bhagwati is concerned that these arguments, if misused, can justify protection. I would agree and argue that the marriage of strategic trade theory and arguments about technological externality can produce a potent ideological brew justifying first-strike economic strategies with protectionism as a core. But that does not make the arguments wrong. To ignore the technological externalities gained by winning a position in sectors that use large volumes of emerging new technologies (a gain that Bhagwati either avoids or denies) requires dismissing the conclusions of a range of economists including Christopher Freeman, Richard Nelson, Paul Winter, Giovanni Dosi, and many others. There are issues here that the trade regime must confront. The best way would be through an agreement on multilateral rules for investment and intellectual property (as Bhagwati and Sylvia Ostry argue); but it may be tough to reach agreement and even tougher to agree that the deals are being kept.

It is unfortunate that Bhagwati's case is not made in the language and style that permits sensible discussion and debate. The debate is an important one, extensive common ground is possible, and the remaining issues for debate are important and fascinating. For the reader intrigued by Bhagwati's views, I would commend his earlier book *Protectionism*. My substantive disagreements remain; but in this previous volume, Bhagwati makes his central case in a more sustained and reasoned way.

University of California, Berkeley JOHN ZYSMAN

In the Name of Democracy: U.S. Policy toward Latin America in the Reagan Years. By Thomas Carothers. Berkeley: University of California Press, 1991. 309p. $29.95.

The major question Thomas Carothers addresses is, Did Reagan administration policy promote or hinder the development of democracy in Latin America? Based on more and less detailed chronological reviews of administration policy toward over a dozen countries, his answer is a "qualified no" (p. 249). By Carothers' accounts, the qualifications are indeed minimal. In El Salvador, for example, administration policy, which strengthened the corrupt and "profoundly anti-democratic" military, had "adverse effects with respect to promoting democracy" (pp. 44–46, 251). Even the administration policies that for Carothers represent the "farthest point of evolution with respect to promoting democracy" do not receive high marks (p. 150). For example, U.S. assistance was only a "minor factor" in the success of the 1988 Chilean plebiscite campaign; and in Paraguay, the U.S. role in ousting Stroessner was "small" (pp. 163, 166).

Carothers addresses two subsidiary questions in less detail. First, why did the Reagan administration adopt such a strong stance toward promoting democracy in Latin America? Most of Carothers' answers are not especially surprising. Among other things, the administration promoted democracy to persuade reluctant members of Congress to approve requests for military aid, to garner support for its anticommunist policies from a public wary of such crusades, and to alter the conditions giving rise to leftist movements and social instability. Carothers also contends that the administration's stance on democracy resulted from a tendency in U.S. foreign policy making for "rhetoric to influence reality": Once the administration publicly linked its policies to democracy, it came under pressure to match its pronouncements with substance (p. 244).

What did the Reagan administration actually meant by democracy in Latin America? is the second subsidiary question of concern to Carothers. He concludes—again, not surprisingly—that the administration's definition was a hyperformalistic, electorally focused, top–down version of what conventional, Western political science understands by the term. The conception was so narrow that it allowed governments exhibiting any measure of corruption, disdain for human rights, and control by military and economic elites to be classified as democracies.

People moderately knowledgeable about U.S. relations with Latin America will not learn much new from Carothers' country-by-country summaries of U.S. policy. Nonetheless, the book is useful in providing an accessibly written, one-stop description of U.S. policy toward many nations over an entire decade. Throughout these discussions, Carothers highlights divisions within the administration, thereby enlivening his accounts considerably and helping him explain subtle policy shifts over time.

The book's strongest and most original chapter deals with political development assistance programs, with which Carothers has intimate knowledge because he worked with Agency for International Development beginning around 1985. This chapter discusses both the National Endowment for Democracy and the spinoffs of Reagan's Project Democracy initiative; and it makes it even more clear why Carothers' assessment of the administration's contribution to democracy in Latin America is largely negative. Taken together, the National Endowment for Democracy and the Project Democracy spinoffs constituted the heart of the democratic development component of administration policy. Yet these programs were minuscule compared to administration spending on military programs; and major portions of them were directed toward such activities as police training and counterterrorism. Finally, Carothers shows why the Reagan administration per se deserved less credit for these democratic centerpiece programs than might seem to be the case. For one thing, many of the Project Democracy spinoffs succeeded in accomplishing their modest goals only because of the informal efforts of individual career officers in the Agency for International Development and other agencies, many of whom had served in their capacities before Reagan's election and remained after he had departed.

Unfortunately, Carothers made a series of research choices that limit his book's value. First, though much of his discussion is based on interviews with past and current U.S. officials, Carothers shares little about these interactions or his respondents (e.g., when the conversations took place, how many people he talked to, or how or why he selected them). As a consequence—and especially since most of those he interviewed declined to speak on the record—readers are forced to remain particularly skeptical of many of Carothers' assertions.

Second, the author chose not to deal with U.S. policy toward Cuba, despite the country's relevance to his topic. For one thing, the administration played relentlessly on the democracy theme vis-à-vis its Cuba policy. For another, Carothers notes, time and again, how the perceived threat from that country was foremost on the minds of administration officials as they sought to develop policies toward many of the countries he does discuss. The author's decision to ignore U.S. policy toward Cuba, even while including treatments of U.S. policy toward countries of inarguably less significance, produced a noticeable and unnecessary gap in the book.

Third, Carothers made some unfortunate and poorly justified choices in his use and treatment of some key concepts. The first was to ground his critique of Reagan's policies in a conventional, Western political science definition of democracy, rather than in a broader, more inclusive understanding of the term that would link politics and policies to social and economic considerations. Second, he failed to take seriously enough his own goal of disentangling the rhetoric and reality of administration policy. Rather, his tendency is often to confound the two.

Carothers' choices on these two matters are related to the book's most serious shortcomings. Often, the author's critiques fall flat and his analyses lack sharpness. There seems to be an odd mismatch between his forceful descriptions of policy development and implementation and his bland evaluations of these processes. At times, it is almost as if one were reading two different books. Finally, Carothers is left with an inadequate stock of tools with which to make sense of his data, forcing him, time and again, to retreat into additional, more detailed presentations of facts and events. The upshot is a book that, in my view, is too long on description and too short on insightful explanation. It is primarily at the moments Carothers' allows himself to distinguish clearly between rhetoric and reality and to make connections between politics and the social and economic contexts in which they are embedded that his book is at its best.

University of Oregon LINDA FULLER

Exploring Realpolitik: Probing International Relations Theory with Computer Simulation. By Thomas R. Cusack and Richard J. Stoll. Boulder: Lynne Rienner Publishers, 1990. 209p. $32.00.

Cosmologists and international relations theorists are faced with the "$N = 1$" problem: while they can study subsystems, only one example of the overall system exists. Computer simulations provide one route out of this conundrum. Just as cosmologists can simulate the aggregation of scattered atoms into stars and galaxies, so computer models of political consolidation can show the development of states from smaller territorial units through conflictual interaction.

Cusack and Stoll present a systematic and detailed analysis of one of the best known of these models, based on Stuart Bremer and Michael Mihalka's "Machiavelli in Machina: Or Politics Among Hexagons" (1977). The idea of simulating political aggregation through territorial conflict is a fairly general one. It had been anticipated by Michael Dacey in 1970, and a similar model for urban segregation was developed simultaneously by Thomas Schelling in *Micromotives and Macrobehavior* (1978).

Cusack and Stoll use the model to explore a number of questions raised by realist theory, in particular, how far power-oriented self-interest creates an "invisible hand" that prevents a multistate system from degenerating into a single "empire." The book includes a chapter-length, amply footnoted discussion of realism, including many of its classical, neo-, and post- variants. The authors find realism to rest on a "fractured" and disparate foundation, though this may be due in part to their casting a net so wide as to include as "realist" virtually all theories that posit the use of "power."

The model contains a variety of parameters such as the accuracy of power estimation, the cost and destructiveness of war, and the variance of power growth rates, as well as several different decision-making rules. This project, though spun off from the GLOBUS simulation, (an elaborate and highly parameterized model), is relatively parsimonious. The analysis is further focused by concentrating almost solely on the question of multistate maintenance. The impact of the parameters is analyzed using statistical analysis on the simulation outcomes following a sophisticated experimental design involving tens of thousands of simulation runs.

The general behavior of the simulation proves remarkably insensitive with respect to most of its parameter values. While each has some influence, by far the most important is the function relating the ratio of the power of two antagonists to the likelihood of victory. In the basic model, multistate systems are reliably sustained only when victory requires a very high ratio; otherwise, about 50% of the simulations coalesce into a single empire.

The experiments with the decision rules, on the other hand, produce a variety of behaviors. Collective security systems and, to a much lesser extent, balancer systems are more likely to preserve states than simple power seeking. While this is a plausible result, the authors note that some theories have argued against collective security efforts as needlessly exposing states to risks. The model thus usefully distinguishes among theories. Expected utility maximization, alliances, and internal revolt are some of the other behavioral variations explored with the simulation.

How realistic is this model? To the extent that one is interested in territorial aggregation determined by relatively simple rules involving comparison, maximization, and occasional expected utility calculations using power, the model is probably quite robust. While there is more to realism than just these calculations, the model provides a systematic exploration of behaviors that are all too often simply assumed in realist theory. Granted, the model says nothing about the Rio Summit or the Uruguay Round; but it probably says a lot about Yugoslavia, Armenia and Ethiopia.

Even the tendency of the model to result in empires may be consistent with observed behavior. The units in the simulation need not be Westphalian states; they require only power, territory, and autonomous decision making. The evolution of a decentralized system into a "universal empire" was observed in subsystems such as Tudor England, fifteenth-century Spain, Tokugawa Japan, and China in 1945–50. The apparent stability of the multistate system we see today may be due in part to prior periods of "imperial" aggregation within its subsystems.

Most of this work was done prior to 1989 and focuses almost entirely on aggregation. While one cannot expect prescience of the authors, it would be very interesting to see greater attention devoted to the issue of dissolution of units through internal instability. Fortunately, since the computer code for the simulation is available from the authors and can be run on any reasonably powerful personal computer, such experimentation is possible for other researchers.

In summary, Cusack and Stoll provide a thorough analysis of a basic model of political power, decision making, and territory. It is neither the first nor the last word on the subject but provides a useful baseline for further experimentation and is to be recommended to individuals interested in formal models of international behavior.

University of Kansas PHILIP A. SCHRODT

U.S. Foreign Policy and the Shah: Building a Client State in Iran. By Mark J. Gasiorowski. Ithaca: Cornell University Press, 1991. 242p. $35.00.

American Presidents and the Middle East. By George Lenczowski. Durham: Duke University Press, 1990. 321p. $40.00 cloth, $17.95 paper.

It is becoming increasingly commonplace among political scientists to recognize the impact of domestic politics on international politics, and vice versa. The two books under review represent the intricacies of this dialectical relationship. The volume by Lenczowski looks at how American presidents from Truman to Reagan have articulated their foreign policy toward the Middle East in light of domestic considerations at home. The book concentrates on the eight presidents' perceptions, priorities, and policies vis-à-vis the Middle East. However, as the author informs us early on, this is not "a psychopolitical analysis of the presidents' personalities and motivations" (p. 5).

Drawing on over 40 published memoirs by presidents, secretaries of state, national security advisors, and ambassadors, as well as Israeli, Egyptian, Iranian, and British statesmen, Lenczowski provides a lucid amount of how post–World War II American presidents viewed the Middle East. He identifies containment of the Soviet threat, survival of Israel, and accessibility of oil supplies as the three constant concerns of American policymakers in the region (p. 281). Lenczowski maintains that the first was considered to be the most important, while the priority of the other two fluctuated depending on the administration in power.

Although *American Presidents and the Middle East* deals with a host of international crises originating in the Middle East (Suez, Mosaddeq's oil nationalization, the civil war in Yemen, Cyprus, and the Soviet invasion of Afghanistan), the central focus of the book is the Arab–Israeli conflict and U.S. involvement in it. The author pays particular attention to the pressures put on successive U.S. administrations by Israel and the American Jewish community. We are told of Truman's "misgivings and caveats" about supporting Zionist objectives (p. 26); Eisenhower's standing up to Israeli pressures; and Johnson's unequivocal support for Israel, which he regarded as a "strategic asset." Furthermore, Lenczowski insists that Nixon and Reagan's desire to appease strong constituencies at home and their Cold War view of the Soviet Union led them to seek closer ties between Israel and the United States. Overall, he considers Johnson as the most vulnerable, Eisenhower the most impervious, president when it came to domestic pressures (p. 280).

Lenczowski's work suffers from three weaknesses. Despite his treatment of certain domestic considerations and constituencies, many important factors and details are left out. The reader is not adequately informed of the compass of political impediments, interest calculations, domestic preoccupations, and partisan congressional agendas each respective president had to grapple with. The most conspicuous omission of the book, however, is any discussion of how presidents tried to build public support for their foreign policies at home in the midst of all these crises. Second, by relying exclusively on memoirs and certain official documents from the executive and the legislative branches, the volume suffers from its sparse source material. Considering the fact that political memoirs are more often than not marred by a sense of self-flattering and self-conceit, the author could have enhanced the value of his work by qualifying and corroborating his sources with other competent accounts that are available. Finally, Lenczowski does not refrain from interjecting his personal value judgements in the book. For example, he brands the Iranian Prime Minister Mosaddeq as "thirsty for power," his oil policy as "reckless" and his sense of nationalism as "misguided" (pp. 36, 64).

In contrast to Lenczowski's book, *U.S. Foreign Policy and the Shah* by Mark Gasiorowski deals with the exogenous determinants of domestic policy. In this original and highly competent book, Gasiorowski examines the nature and progression of the "cliency" relationship that emerged between the United States and Iran in the aftermath of the 1953 coup overthrowing Premier Mosaddeq. After defining *cliency* as "a mutually beneficial, security-oriented relationship between the governments of two countries that differ greatly in size, wealth, and power" (p. 2), the author provides a promising theoretical framework for how international cliency relationships can influence the domestic politics of client countries.

Gasiorowski's main argument is that economic aid and security assistance provided by patron powers to their client states empower the latter to seek autonomy from civil society. Lacking any societal constraints, the state can begin to defy all demands for political participation, thereby leading to serious unrest, instability, and even revolution. Henceforth, the initial intention of patron powers to maintain political stability in their often strategically located client states may inadvertently produce the opposite results.

In seven detailed chapters, Gasiorowski examines the nature of state formation in Iran over the last four decades. He maintains that the formation of the cliency relationship between the United States and the Shah and the rapidly augmenting oil revenues in the 1960s and 1970s helped to initiate and sustain a highly autonomous Iranian state in the prerevolutionary period. Gasiorowski contends that a mere 10 years after the overthrow of Mosaddeq, an autonomous Iranian state was firmly in place (p. 187). This state's premeditated use of repression as its most important autonomy-enhancing mechanism contributed to the advent of the Islamic revolution.

In his analysis of the 1953 coup, which can quite properly be regarded as a turning point in modern Iranian history, Gasiorowski has left no stone unturned. Utilizing a wealth of recently declassified documents from the British Public Records Office, the U.S. National Archives and the U.S. Department of State, in addition to over 70 interviews with British, American, and Iranian officials and activists involved in the event, Gasiorowski has produced by far the leading study on the subject. Through his meticulous use of primary and secondary sources, Gasiorowski has authoritatively augmented our knowledge of that tempestuous event and its tragic aftermath.

This book, however, is not without its shortcomings. Many political economists would argue with Gasiorowski's assertion that cliency was more important than oil as a determinant of state autonomy. It is highly doubtful whether the state could have sustained its autonomy from civil society to such a degree and for such an extended period of time without the petrodollars. One should not lose sight of the fact that the financial largess of the state, provided to it by oil exports, enabled it to coopt an already-subdued polity. Second, the author's identification of the National Front (led by Mossadeq) as the main political organization of the modern middle class (p. 84) may be somewhat of an oversimplification, considering the eclectic constituency base of that party, which drew support from disgruntled *bazaaris* and the Qashqai tribe, in addition to the urban secular middle class.

Lenczowski and Gasiorowski respectively demonstrate that a great power can be pressured by a small ally and its vocal domestic supporters and that patron states should beware of enabling client states to become highly autonomous. Their virtue resides in the fact that by making the confluence of domestic and international policies problematic, they can provide all students of foreign policy with much food for thought.

Syracuse University MEHRZAD BOROUJERDI

Pathways from the Periphery: The Politics of Growth in Newly Industrializing Countries. By Stephan Haggard. Ithaca: Cornell University Press, 1990. 276p. $12.95 paper.

Stephan Haggard seeks to compare the development policies of the East Asian "tigers" (Korea, Taiwan, Hong Kong, Singapore) with those of two Latin American countries (Brazil and Mexico) and to account for the differences. More than that, however, he wants to explain the policy changes adopted in each of the countries during the postwar era.

The East Asian strategy of export-led industrialization has been much more successful than the import substitution strategy (ISI) adopted throughout Latin America. But Haggard points up an interesting paradox. Among the reasons ISI was instituted in Latin America was the belief—shared by technocrats and representatives of the organized working and middle classes—that ISI in a protected national market would allow for higher average wages and, consequently, greater distributive equity and less dependence on forces beyond national control. Export-led industrialization, in contrast, was thought to entail severe wage restraint and dependency. Yet the outcomes have been the diametric opposite of these expectations: the data cited by Haggard show clearly that all but one of the "tigers" score higher than the Latin Americans on Gini measures of distributional equity and on quality-of-life indexes such as average level of education, longevity, and infant mortality rates.

Haggard, in seeking to unravel this paradox, is mindful of the impact of international forces and pressures. He notes, for instance, how both Korea and Taiwan actually began their industrial development along ISI lines but then shifted to export-led industrialization as declining U.S. economic assistance made the former policy less viable. However, his explanatory framework prioritizes domestic factors, since no theory of the international system can explain policy differences between developing countries whose situations are otherwise similar. For Haggard, the key domestic factor is the structure and capabilities of the state: in some countries, "political elites have inherited or built organizational structures that significantly constrain the ability of societal actors to achieve their political and economic objectives" (p. 3). This is important because successful development requires more or less drastic policy changes to adapt to changing world conditions or redress demonstrated failures. But any strategy of development creates social forces with a stake in it; and if those forces are well organized and politically mobilized, their tendency is to use their political power to block changes that would adversely affect their interests.

Consequently, the study of development policy becomes the study of efforts by ruling elites to build supporting coalitions in the context of a set of state institutions "that provides differential incentives for groups to organize. Because of variations in institutional structure, political elites differ in their organizational capabilities and the instruments they have at their disposal for pursuing their goals. . . . It is at the intersection of choice and institutional constraint that political explanations of economic growth must be constructed" (p. 4). Hence, Haggard eschews rational choice approaches to explaining policy output, although he admits that they can shed light on the actions of the contending societal groups.

This position reduces to restating development policy choice as a collective action problem along the lines discussed by Mancur Olson: when societal groups can organize to influence policy, they form coalitions to secure benefits for themselves and thereby restrict both the state's ability to choose among available policy options and the coherence of policy itself. Haggard thus returns us to the argument of the 1960s (which much of the subsequent literature has been at pains to refute) that development may be less than fully compatible with an early broadening of democratic norms and institutions. To be sure, Haggard is not an advocate of all political authoritarianism, since he knows that such regimes may be penetrated by dominant groups with partial interests. In Latin America, where this penetration has been frequent, "the association between industrial strategy and authoritarian rule . . . appears to be weak" (p. 255). Rather, the authoritarianism he favors is the sort labeled "state autonomy." The most effective development policies, he avers, are products of "those regimes which limit autonomous political organization and public contestation," whether through the direct use of state power or via corporatist controls (pp. 44–45).

It seems to me that Haggard is tacitly assuming that the authoritarian order is led by people who are both aware of, and devoted to, the public good—or at least have an interest in a form of development that will benefit a wide range of social sectors. It also seems that he has presumed a high degree of administrative expertise at the disposal of these leaders. (Although the cases he has chosen give him no reason to make the point explicitly, he plainly excludes personalist authoritarian regimes like, say, Mobutu's in Zaïre or Somoza's in Nicaragua.) But what about communism, which would seem to meet all the requirements of a "desirable" form of authoritarian control? To say that communism failed owing to its rejection of markets and fear of the international economy begs the question; for that answer implies the significance of leaders' *beliefs* about the nature of the world and the kind of society they want to build, and Haggard does not discuss such beliefs, how they are formed, or how they are affected by international influences. While he makes mention of "economic ideologies" (pp. 46–47), these are professional understandings of economics and policy, transmitted through international professional networks and institutions. They are not the worldviews and moral interpretations of social reality that we usually mean when we employ the term.

There are, of course, many other reasons why committed democrats will not rush to embrace Haggard's claims. One is the present coincidence between the restoration or strengthening of Latin American democracy and the most serious effort yet undertaken in the region to undo the excesses of statism-cum-ISI. A second is the suspicion that the political practice of subordinate social groups may be informed by stronger democratic impulses and a better sense of the general welfare than we have assumed. Richard L. Sklar is attempting to build a theory on just this basis ("Developmental Democracy," *Comparative Studies in Society and History* 29[1987]). His efforts remain at a preliminary stage of exposition; but, in my opinion, they are already fruitful enough to give us pause.

Haggard's book should serve well as a text for graduate and advanced undergraduate courses in development policy. It may not be the last word on the subject. Apart from its uncertain view of political democracy in developing countries, its relative inattention to class forces deprives it of explanatory power in regard to underlying processes of social and political change. But students will learn the truth about the industrialization of East Asia—that is, that this process depended and still depends on heavy state intervention—and will profit from the comparisons between East Asia and Latin America. They will also be stimulated to reflect on the thought that the shortest distance between the objective of equitable development and the result may not be a straight policy line.

Dartmouth College DAVID G. BECKER

Peace and War: Armed Conflicts and International Order 1648–1989. By Kalevi J. Holsti. New York: Cambridge University Press, 1991. 379p. $54.50 cloth, $15.95 paper.

Focusing upon foreign policy issues, particularly those over which states fight violent conflicts, Holsti attempts a systematic treatment across three-and-a-half centuries. This in itself is a stupendous exercise. Link the size of this empirical effort with the amount of knowledge required to make the analysis meaningful politically and the conclusion must be that this is a major work of scholarship indeed.

The book combines empirical research on international relations with diplomatic history; assessment of foreign policy treaties and their aftermath; and the strategic outlook of statesmen, that is, political realism with an admixture of idealism. Further, study of the assimilation of defeated hegemonic states is an important dimension of this work. Holsti carries the examination of the Peace of Westphalia (1648) and the Treaties of Utrecht (1713), each of which ended and began a new system, through the more familiar Congress of Vienna (1815), Peace of Versailles (1919), and the San Francisco treaties that led to the establishment of the post-1945 bipolar system. Contained within these portentous treaty constellations are the issues over which the prior system collapsed and a new system was born.

A great attraction of the book is how it unearths the visions of the architects of statecraft like Stanhope and Talleyrand, Metternich and Castlereagh, as well as those of the rogues like Hitler and Stalin. By staying close to the individuals who made foreign policy, this assessment of international relations possesses authenticity.

Perhaps the highpoint of Holsti's writing comes with his evaluation of the thought of Woodrow Wilson. Wilson was enigmatic. He was magnanimous and punitive. He was interventionary (in Latin America) yet the opponent of power politics at Versailles. He was a dreamer yet ruthlessly disregarded the concerns of Lloyd George and Clemenceau. He was a great proponent of public opinion yet remained ignorant of the opinion at home and in the Senate that would kill his grand idea. Wilson was the hero of self-determination and democracy at a time when neither could be fully effected and when the greatest danger still lay with old-fashioned nationalism and dictatorship. Still today, it is Wilson, more than perhaps any other diplomatist, who has left an imprint on the theory of international relations, showing that a world of democracies is less likely to be war-torn than its contrary.

According to the descriptive statistical findings Holsti reports, the place of territory in the disputes of governments has declined. Territory as an issue has gone from a high of about one-quarter of the total disputes in the seventeenth and eighteenth centuries to less than one-tenth in the late twentieth century. This finding could lend support to the notion of the demise of the territorial state. But if one combines all of the other categories of issues involving territory—namely, strategic territory, territory (boundary), national liberation as state creation, national unification and consolidation, secession as state creation, and territory per se—the inference is quite different. Together, territory in this larger sense accounts for about a third or more of all issues throughout the period. Territorial dispute remains, in its many forms, the most problematic issue facing states today.

Despite the great conceptual and empirical difficulty of determining how many foreign policy issues pertain and which issues are more central in any given dispute, Holsti possesses a data set that, when further analyzed, may produce other important insights. For example, the relationship between the size and seriousness of an armed dispute and the issue or issues at stake is of great interest. It is to be expected that certain core issues regarding role and status, as well as security, will predominate in the most violent and major of confrontations. Holsti has not yet tested this hypothesis, though he might.

Combined with its astute investigation of the attitudes of the statesman, the empirical findings of this pioneering work make this book rich ground for subsequent theory development.

Elaborated in the final chapters is the difference between the system of the advanced industrial states (virtually the central system, or the great-power system, in other parlance) and the system of states in the Third World. Surely, Holsti is correct that governments within each of these systems or subsystems pursue some divergent foreign policy aims. His assertion that the former is preoccupied with avoidance of nuclear war while the latter is more focused upon the problem of the security of the state and the survival of the governmental regime is undoubtedly correct. Though nuclear proliferation may yet blend these heretofore divergent subsystemic anxieties, nevertheless, to the extent that the types of conflict differ for each subsystem, so may the reasons for war.

A full incorporation of other approaches to world politics with the splendid empirical assessments of this volume promises many further insights into the cause and results of aspirations to world order.

Johns Hopkins University CHARLES F. DORAN

Dominoes and Bandwagons. Edited by Robert Jervis and Jack Snyder. New York: Oxford University Press, 1991. 299p. $37.50.

During the Cold War, the domino theory and a series of related metaphors (e.g., "rotten apples in a barrel," "weakest links in a chain," etc.) lay at the heart of American foreign policy in small countries around the world and largely accounted for the pattern of U.S. intervention in Third World revolutions, especially in Vietnam and Central America. The domino theory held that decisive action had to be taken to prevent the fall even of small countries of little or no intrinsic political, economic, or strategic significance lest the fall of such countries set in motion a chain of events inexorably leading to communist takeovers in country after country throughout entire regions. Indeed, the consequences might even be global, for falling dominoes might undermine the credibility of U.S. commitments elsewhere, leading to a "bandwagoning" effect as neutral states and even allies rushed to accomodate the apparently inexorable communist wave.

Despite its critical significance to American foreign policy in the Cold War—and indeed (as Snyder and Jervis show) to the policies of great powers throughout history—there have been remarkably few systematic, scholarly analyses of the domino theory or empirical/historical evaluations of its validity. This excellent book has gone a long way to fill the gap, though there still is a need for detailed historical case studies (e.g., for Vietnam.

In their introductory and concluding chapters, Jervis and Snyder provide historical and theoretical overviews of the domino theory, bandwagoning, and related metaphors. The other essays, all of them first-rate, include analyses by Stephen Walt and Deborah Welch Larson of bandwagoning, balancing, and alliance formation; Douglas J. Macdonald's discussion of the domino theory during the Truman administration; Ted Hopf's assessment of the Soviet Union's reaction to the domino theory; and other analyses of Soviet policy in the Third World by Douglas Blum, Richard Herrmann, and Milan Hauner.

While there are some differences among the contributors, most are very skeptical of the domino theory. The most important overall conclusion is that while, historically, there have been some circumstances where dominos have fallen or states have jumped on bandwagons (e.g., Eastern Europe, 1989–91), much more typically, the theory fails to predict the behavior of states. There are a variety of reasons for this, including the inapplicability of events in one country or region to very different circumstances in other countries or regions, as well as the power of nationalism in creating obstacles to expansionist states or ideologies. Beyond that, one of the most important arguments of the authors (especially Jervis, Snyder, and Walt) is that the domino theory is more often a self-denying, than a self-fulfilling, prophecy. When the first in a feared chain of dominoes fall (as happened in China in 1949, in Cuba in 1959, and in Vietnam and Angola in 1975), neighboring states are much more likely to balance than to bandwagon—that is, they shore up their alliances, rebuild their military power, take preventive action to avert revolution, and so on. Moreover, as Hopf demonstrates, after Vietnam, Soviet behavior in the strategically significant areas of the world was more cautious, rather than more aggressive; for Moscow correctly anticipated that the West would redouble its efforts to prevent any significant shift in the global balance of power.

For these reasons, then, the fall of Vietnam in 1975 had little lasting effect on the Cold War conflict. Some neighboring small states of no intrinsic significance quickly succumbed to communism (Laos, Cambodia); and there was a brief surge of Soviet support for a handful of Third World revolutions elsewhere, especially in Africa; but there was no widespread collapse of dominoes. Even in Asia, none of the large or truly important states, like Japan or India, were affected; and there was certainly no loss of U.S. credibility or rush to accomodate elsewhere, as in Europe or the Middle East. Jervis persuasively explains why: "The domino theory exaggerates the role of reputational interests in determining the outcomes of confrontations and the inferences observers draw and commensurately underestimates the importance of intrinsic interest. . . . States seem able to separate peripheral from vital interests" (p. 42).

There is little reason to think that assessments of the domino and bandwagoning theories have lost their relevance in the post–Cold War era. As the authors demonstrate, in all historical eras, there has been a tendency for dominant powers to adopt domino theories in order to justify interventionist behavior. Precisely because of the collapse of the Soviet Union, the United States is militarily even more dominant than ever. Therefore, it is still vulnerable to the imperial temptations that ultimately account for the domino theory.

State University of New York, Buffalo JEROME SLATER

The New European Community: Decisionmaking and Institutional Change. Edited by Robert O. Keohane and Stanley Hoffmann. Boulder: Westview, 1991. 208p. $49.95 cloth, $14.95 paper.

Euro-politics: Institutions and Policymaking in the "New" European Community. Edited by Alberta M. Sbragia. Washington: Brookings Institution, 1992. 303p. $32.95 cloth, $14.95 paper.

The New European Economy: The Politics and Economics of Integration. By Loukas Tsoukalis. New York: Oxford University Press, 1991. 333p. $52.00.

The titles of these three volumes have in common the word *new*, in recognition of the momentous changes that have taken place in the European Community (EC) since the mid-1980s. The writings found in them may be characterized as representing "new thinking" on the institutions, policymaking and economics of the EC as of the early 1990s. With the exception of Stanley Hoffmann, the writers are also "new" to the debates regarding the nature of the beast, debates that raged in the 1960s and then exhausted themselves during the period of Eurosclerosis of the 1970s and early 1980s.

The volume edited by Hoffmann and Robert Keohane contains essays written in 1989 and 1990 that sought to gain better theoretical purchase regarding the genesis and political meaning of the 1986 Single European Act (SEA). The other two works were produced during the period of the intergovernmental conferences (1990–91) that led up to the Maastricht summit of December 1991.

Although the discussion brings fresh perspectives to bear on new phenomena, the books edited by Keohane and Hoffmann and by Sbragia, written mostly by political scientists and addressing essentially political topics, are haunted by the specter of the earlier debates, particularly in their frequent uses of the terms *neofunctionalism, neorealism, intergovernmental,* and *supranational.* The best job of sorting these terms out is done in the Sbragia collection by David Cameron in his essay "The 1992 Initiative: Causes and Consequences." The first two terms are treated by Cameron as labels for theoretical perspectives, while the latter two are applied to EC institutions. Basically *neofunctionalism* highlights the role of "supranational institutions," particularly the EC Commission, in making decisions that in Ernst Haas's terms, "upgrade the common interest" among member states. *Neorealism* emphasizes the role of "intergovernmental institutions" (the Council of Ministers and, esp., the European Council) in arriving at decisions that express the "lowest common denominator" of agreement among members.

In their introductory essay, Keohane and Hoffmann argue that none of these terms exactly applies to how decisions are made in the EC today. Neofunctionalism produced the "spillover hypothesis" that predicted a strengthening of supranational institutions as a result of the consequences of EC policies pursued in one economic sector for the performance of the member countries' economies in other sectors. Spillover and the even more economically deterministic "international political economy" hypothesis would have predicted a strengthening of supranational institutions during the economic crises of the 1970s to address the dislocations produced by the energy crises and national macroeconomic policy failures. They acknowledge that development of the European Monetary System (EMS) in the late 1970s provided the hypotheses partial support. However, swifter progress toward the internal market and a full Economic and Monetary Union (EMU) should have occurred but did not. They do not make a similar effort to weaken a third hypothesis, which they label "preference convergence," namely, the prediction that decisions will be reached only if the preferences of member states converge. This stems from neorealist premises and stresses intergovernmental institutions. Nor do they suggest that the convergence which occurred in 1984–85, making the SEA possible, was brought about by factors common to the principal actors. This sets the stage for the next essay, by Andrew Moravcsik, which gains attention from Cameron and other writers in the Sbragia collection.

Moravcsik makes a strong case for the preference convergence hypothesis, couched in the sense that member states can be expected to reach only the lowest common denominator of agreement. In the case of the SEA, the principal actors on which a neorealist would focus are West Germany, France, and Britain. Moravcsik details the preferences of the Mitterrand administration and the Thatcher government that converged on the SEA, a package agreement that combined the elimination of barriers to the EC internal market with relaxation of the unanimity convention regarding Council of Ministers voting on most pieces of internal market legislation. Prior to the agreement, France and West Germany had acquiesced in a budget agreement that went a long way toward satisfying Britain's complaints about EC funding burdens. While there was substantial agreement among the three as to the desirability of the market-freeing aspects of the SEA, Moravcsik's analysis makes it clear that France and Germany gave ground on the budget issues (esp. France, given the decision to lessen the Common Agricultural Policy share of the budget), while Britain gave ground on the voting procedure issue. This suggests more an upgrading of the common interest than the arrival at the lowest common denominator of agreement among the three. However, this is not to disagree with Moravcsik's more general contention that member governments acting within intergovernmental institutions played the decisive role in the process.

Other writers in the two volumes are more reserved on that score, since they pay more attention to the role played in the formulation of the SEA by the European Commission, as the leading supranational institution. Cameron argues that the commission was laying the groundwork for the SEA with specific internal market proposals during the early 1980s when the member governments were confining themselves to broad statements of intent, rather than to the concrete actions that began with the Stuttgart summit in 1983. This was before Jacques Delors became commission president at the beginning of 1985. Cameron appears to agree with Moravcsik that Delors's role could not have been decisive before the June 1985 Milan summit. But that was the point at which the package deal began to take shape, and Cameron believes that Delors played an important strategic role in bringing the deal about. In more general terms, Peter Ludlow, in his balanced essay "The European Commission" points to the "educative" role of commissioners in helping governmental leaders to "learn" of the necessity for common action to be taken to deal with common problems, a perspective that has neofunctionalist overtones. Cases in point, in addition to Delors in the latter 1980s, were the roles played by

Commission President Roy Jenkins in helping to bring about consensus in support of the EMS in the late 1970s and by Commissioner Etienne Davignon in fostering, in the early 1980s, industrial policy cooperation among major European companies that later supplied influential support for the SEA.

Efforts are made by other writers to transcend, or at least escape, the intergovernmental/supranational polarities as conceptual tools. In his essay "The EC Council," Wolfgang Wessels makes a distinction between the constitutional decisions and package deals made at the European Council summit meetings and the ordinary work of the Council of Ministers, involving "iron triangles" familiar to students of the American political system. In the Sbragia collection, *Europolitics*, B. Guy Peters makes a similar distinction in his essay "Bureaucratic Politics and Institutions of the European Community." According to Peters, there is "the level of very visible *haute politique*, in which the leaders of the member nations meet as the European Council to negotiate among themselves over the future shape of Europe," and a second level, that of "the bureaucracy functioning within the Commission" (p. 76). Wessels subscribes to the idea that innovative acts of the European Council, which others take as evidence of intergovernmentalism, are responses to economic imperatives that the heads of state and government take in common. Hence, the European Council has become a supranational body that upgrades the common interests. For his part, Peters resuscitates Martin Heisler's 1873 European Polity model as one appropriate for how intergovernmental bodies make decisions in the new era of qualified majority rule; that is, the motivation to join in common decisions and avoid isolation is stronger than under the unanimity rule. Thus, both the European Council and the Council of Ministers are more apt to reach decisions promotive of integration than to protect narrowly defined national interests.

More transcendent is the attempt by Alberta Sbragia in her concluding chapter to expand the concept of federalism to characterize what the EC is becoming in political/constitutional terms. According to Sbragia, the idea of federalism that has prevailed in the debates about the EC has been too narrowly influenced by the essentially unique brand of federalism found in the United States. Germany, where the member states are more powerful than those in the U.S. federal system, would provide a better model for understanding the EC today and for projecting it into the future. When theorists friendly to European integration have thought about an end-state, they have found it difficult to escape something like the U.S. version of federalism. As the EC moved further and further away from that model, especially as its intergovernmental features became more pronounced, such theorists came to despair of their desired goals' ever being achieved. Yet a looser, more flexible, concept might have served them better, in Sbragia's opinion.

Whether these imaginative efforts to go beyond the old debates will take on adherents would be impossible to predict at present. The rejection of the Maastricht Treaty by Danish referendum has highlighted the problem of the "democratic deficit," which Shirley Williams discusses with considerable insight in "Sovereignty and Accountability" in the Keohane and Hoffmann collection. Perhaps the problem of how to associate the mass public in what has been until now an essentially elitist set of institutions will come to preoccupy more students of the EC in the next decade; or perhaps it will be the problem of reconciling the internal market with the need to protect the "losing" regions, economic sectors, and social classes—a problem addressed in essays by Gary Marks and Peter Lange in the Sbragia volume. Such problems will likely be exacerbated with EC expansion, about which little of substance is said in either volume. Other supranational institutions are also considered: the European Court of Justice by G. Federico Mancini, a judge on the court (in Keohane and Hoffmann) and Martin Shapiro (in Sbragia) and the EMS in a sharply analytical essay by John Woolley (in Sbragia). Unfortunately, only a mention of these fine essays is possible here. In short, both volumes provide for political scientists a wealth of useful interpretations of recent complex and largely unanticipated developments.

For the political scientist who has difficulty mastering the complexities of EC economic policies, *The New European Economy* by Tsoukalis will be indispensable. While essays in the Sbragia volume by Cameron, Woolley, Marks, and Lange are written in a political economy mode by political scientists, they stress the political, more than the economic, factors operating in EC economic policymaking. Tsoukalis, an economist, provides a clearly written account of the economic factors at play while continually calling attention to political antecedents and consequences. As economists are inclined to do, he opposes the "market" and the "state," assigning efficiency to a free market and stability and equity to politics. His own preferences are by no means exclusively on the side of economic efficiency. Whereas writers in the Keohane–Hoffmann and Sbragia books concentrate for the most part on the issue of whether power is becoming more centralized in EC institutions, Tsoukalis addresses what may well be the more fundamental issue: Will the EC remain simultaneously a market-liberating "economic giant" and an economy-shaping "political dwarf"? He interprets the SEA and the steps toward the EMU as means by which the member states have been depriving themselves of the instruments of economic control without allowing the institutions of the EC to acquire compensating control measures of its own.

A "spillover hypothesis" that could be derived from Tsoukalis' analysis would suggest (1) that as the distributive consequences of the SEA reveal themselves, at least in times of recession, to be negative for competitively weaker firms, economic sectors, regions, and member countries, interest groups and governments affected will press for the acquisition of redistributive powers by the EC and (2) that as the EMU shows itself to be an effective means of maintaining price stability while removing the capacity of exchange rate fluctuation and the countercyclical uses of fiscal policy by member states to maintain stable economic growth without high unemployment levels, affected interests will seek a transfer of fiscal capacity from the national to the EC level. In both scenarios, EC powers would expand accordingly.

Tsoukalis' analysis shows that such neofunctionalist expectations remain dubious in the present context. Neither the social policy nor the revenue-gaining powers of the EC are as yet very impressive. Member governments are retaining their holds on both, even though their capacity to wield these policy instruments for purposes of redistribution and economic stabilization have diminished substantially since the 1960s. In other words, a reading of Tsoukalis leaves one skeptical about whether the SEA and EMU are good examples of neofunctionalist spillover. More likely they represent a "spilling out" of political capacity into a void. If we follow Tsoukalis, then, without a strong Eurostate there can be no effective regional or industrial policy, no "Fortress Europe," no social corporatism, and (very likely) no public acceptance.

University of Missouri, Columbia DAVID M. WOOD

Adjusting to Reality: Beyond "State Versus Market" in Economic Development. By Robert Klitgaard. San Francisco: International Center for Economic Growth, 1991. 303p. $22.95 cloth, $11.95 paper.

Robert Klitgaard possesses a genuine talent for communicating with broad audiences about serious matters of public policy. As with his previous, highly successful *Tropical Gangsters* (1990), *Adjusting to Reality* explores themes of great interest to scholars and policymakers in an engaging and accessible way. In the case of *Tropical Gangsters,* practical problems of development assistance motivated the work; in this case, it is the problems associated with structural adjustment.

There is a serious analytic core to this book. The argument is grounded on the theory of market failure, in particular, the problems arising from imperfect information. Given market failure, Klitgaard emphasizes, it makes little sense to talk of a return to the market or of an abandonment of the state. In making this point, Klitgaard goes further than have others to offer specific steps that governments can take so that "government failure" does not merely replace "market failure" in the making of public policy. In making his case, Klitgaard draws from a variety of his own experiences. Most centrally, he works with materials from Bolivia and various parts of Africa. The skill with which he employs case materials imparts an immediacy and freshness to this work.

While there is thus much to admire in this book, there is also much to criticize. While engagingly written, well grounded analytically, and communicated with a sense of immediacy, the book comes across as superficial. There are several reasons. One is endemic to the development field—the tendency to advance single-factor explanations. Once it was population; at another time, the terms of trade; most recently, parasitic governments. In *Adjusting to Reality,* it is imperfect information. One can legitimately ask, Even if a government did all that Klitgaard says it should, would the economy grow? The answer is that we do not know, because so many other factors, besides the absence of high-quality information, are at work. Klitgaard's broader point—that there remains a broad role for governments, even in market-based economies—is, of course, a good one and well worth making. But by narrowing the argument to a single source of market failure, he threatens to make costly information the flavor-of-the-week in a field already too given to faddism.

A second reason the work will not ring true to many is that it is dangerously apolitical. In this book, there are decision makers but not politicians; there are policymakers but not presidents or generals who seek to use their control over office to build coalitions, win elections, or undertake those other steps that must be taken in order to stay in power. Suppose it true that if a government followed the recipes offered in this book, development would occur. One might still wonder whether all decision makers would be equally motivated to follow them. The factors that would influence the eagerness with which they would do so would be precisely those factors of interest to political scientists. They have been studied by such scholars as Ronald Rogowski, Jeff Frieden, Joan Nelson and company, Robert Kauffman, Steph Haggard, and many others—scholars who have focused on the *politics* of structural adjustment. They are not analyzed here.

The two criticisms can perhaps be more persuasively registered if they are combined. Suppose we focus on other sources of market failure—public goods, externalities, or imperfect competition resulting from the large size of "special interests." Then it would be inconceivable for politics not to become the central focus of the book. The policy counsel and technocratic solutions provided in this volume would be revealed for what they are—well-meaning, often clever, occasionally astute, but far too often politically unsophisticated.

I would use this book with undergraduates. It might be useful, as well, in public policy programs. It would not work well in graduate courses.

Duke University ROBERT H. BATES

France after Hegemony: International Change and Financial Reform. By Michael Loriaux. Ithaca: Cornell University Press, 1992. 314p. $36.50.

Even a dabbler in French political economy knows that before you can properly claim to know anything at all, you must first master the details, all of them, eccentric and obscure though they are and pointless though at first they seem. Michael Loriaux has mastered the details. He has also mastered their historical and political context and framed the whole with a credible theory.

Loriaux sets out to map the Labyrinth—that maze of relations among state, finance, and economy that marked the development of France after 1945—and not only to map it but also to show how it developed; why it could succeed for a time; what pressures it came under as the world economy unraveled in the 1970s; and why, ultimately, in an irony of ironies, a Socialist government was compelled to adopt liberalizing reforms beginning in 1983. He succeeds brilliantly at these tasks, providing a historical narrative that is a dense, sustained synthesis covering perhaps two-thirds of this work.

Ariadne's thread for the whole enterprise is a single concept, that of an "overdraft economy," characterized by assured access to credit for major economic agents. Loriaux argues that such an economy arose under the "hegemonic protection" of the United States, survived for a while, became sclerotic, and eventually crumbled as American protection was removed.

Whereas others, including myself, have emphasized the modernizing role of French state mechanisms from Monnet through Barre, Loriaux compels attention to a darker side. His focus is on the "insurance functions" of the state and on the "moral hazard" that arose as the state lost its way in its own net of implicit guarantees of economic and social position. In a country that after 1945, allowed state-influenced institutional credit sources to dominate it and then devolved social security functions onto firms, the presumption of assured access to credit invited stasis. There were modernizing institutions, to be sure; but Loriaux argues that on balance, the system inhibited economic and technological adjustment and made France more vulnerable to the shocks of 1973 and after than it otherwise would have been.

As at the level of the firm, so at the level of the state. France itself was able to institutionalize its internal overdraft economy only because the United States was prepared to grant analogous privileges to the whole nation. This we did, at first, out of fear of the French Communist party. From the Marshall Plan through 1968, France was able to close the gaps between the expected profitability and competitiveness of its industries (and other ventures, such as the First Indochina War) by relying on the United States. But when the United States turned inward, adopting a policy of "predatory hegemony" under the strains of Vietnam, France was cut loose. It then had to begin to face the hard budget constraints imposed by oil shocks, then European monetary coordination, and also, presumably, by the unwillingness of the Germans to extend credit on quite such easy terms.

Thus we come to the paradox of the 1980s. A Conservative government, that of Barre, had tried and failed to restore France's competitiveness through massive use of the traditional techniques of industrial and financial intervention. A Socialist government took two years to figure out just how hard the budget constraint had become. Then came the crisis. What to do? The only solution was to establish a capitalist financial market and an asset-based banking system in France, for the first time since the 1930s, so as to reestablish a buffer between the demands of society for insurance and the true capabilities of the state to provide it. This the Socialists did. It was an elegant (if not wholly effective) exercise in the politics of necessity, and Loriaux's write-up is an elegant exercise in the analysis of political choice.

Loriaux is highly critical of the overdraft economy from its inception to its end. In this, it may be that he stretches his point. He evidently believes that a restoration of free financial markets, to have begun with Mendes-France's proposed, but spurned, monetary stabilization of January 1945 and followed thereafter with a program of monetary orthodoxy à la Jacques Rueff, would have yielded a more competitive and resilient French economy in the long run. How can we be sure? After all, the French economy did grow reasonably strongly through the 1950s and 1960s, while maintaining at least a simulacrum of social stability. It had not achieved this when strong and free financial markets existed, in the 1930s. Perhaps moral hazard, overinsurance, and inflation were prices that had to be paid to achieve growth, prosperity, and some modernization in the delicate political environment of postwar France. The politics of necessity is a tricky business, and it is a little unfair to celebrate the outcomes when they conform to orthodoxy while condemning them when they do not.

Nor is it certain that the overdraft economy per se was a recipe for failure. Other overdraft economies existed—notably in Japan, Korea, and Finland—and (as Loriaux acknowledges) at least the first two of these showed capacity for internal reform in advance of crisis. Perhaps to create such an economy was a rational response to the opportunities provided by American hegemony, a chance to get ahead while the getting was good. And perhaps France's problem was mainly a matter of misplaced investment choices. In particular, where Japan and Korea zeroed in on the American market, France chose to adventure into the supply of weapons, capital goods, and engineering projects (notably, subways, airports, ports, and mines) to the developing world. It might have been a reasonable bet that expanding U.S. hegemony would have sustained the market for such exports; but it happened, instead, that American hegemony contracted. France, having made choices that were not so much wrong as unlucky, found itself and its opportunistic system among the first victims of a changing world.

These, however, are issues of interpretation. The pleasure of Michael Loriaux's achievement is that he makes possible a discussion of such issues on an exceptionally solid foundation, both of historical fact and reasoned theoretical judgment. *France after Hegemony* is an admirable book and a satisfying read.

University of Texas, Austin JAMES K. GALBRAITH

Inadvertent Escalation: Conventional War and Nuclear Risks. By Barry R. Posen. Ithaca: Cornell University Press, 1992. 280p. $36.50.

With the end of the Cold War, many policy-relevant books in security studies became of purely historical interest. Barry Posen's study of the process by which conventional conflicts can escalate to nuclear war avoids that fate, at least in part, because of its explicit theoretical focus. The body of the book is a very detailed examination of projected military operations in a major war between the Warsaw Pact and NATO, a potential conflict that has joined the communist regimes of Eastern Europe and the Warsaw Pact in the proverbial ashcan of history. Fortunately, the analytical tools that Posen develops and exercises in the study can be applied to a variety of regional conflicts in the post–Cold War world.

Posen's central argument is that a large-scale conventional war between two nuclear-armed adversaries is likely to escalate to the use of nuclear weapons despite the initial intentions of both sides to avoid such escalation. Three factors cause such tendencies. First, Posen argues, the "security dilemma" in international relations (in which actions taken by a state to defend itself unintentionally threaten others) also applies to the conduct of conventional war. Political leaders may issue orders to execute conventional military operations for defensive purposes, unaware that such operations threaten or damage the nuclear forces and command-and-control of its adversary. If that adversary believed that it was in danger of losing its nuclear capability, it might choose to launch a preemptive or limited nuclear attack in a desperate effort to avoid the effects of such vulnerability. Second, the organizational interests of the military exacerbate this dilemma: militaries often favor offensive doctrines and plans, since offensive operations tend to increase organizational power, simplify planning problems, and enhance prestige. In addition, military organizations, like most organizations, greatly value their autonomy from outside influence, which can seriously inhibit the sharing of details about operational plans with civilian authorities. Third, Posen borrows the concept of the "fog of war" from Clausewitz: the inevitable difficulty in understanding communications and intelligence in the midst of combat encourages inadvertent escalation both by making detailed central control over operations problematic and by increasing the fear that an adversary might take advantage of such confusion by launching a surprise attack.

The rest of the book presents Posen's assessment of the likely conduct of conventional military operations in a major war in Europe in the mid-1980s. There are four case studies. First, Posen analyzes a large-scale air war between the two alliances and argues that NATO conventional attacks on Warsaw Pact air defenses and command-and-control systems in Eastern Europe would have eventually increased the vulnerability of the strategic command-and-control systems in the Soviet Union. Second, an assessment of the ground forces campaign leads to the conclusion that NATO armored forces could have defended the central front, thereby avoiding deliberate escalation to nuclear war. Third, Posen examines the potential for escalation if NATO naval forces, following the U.S. maritime strategy, had attacked Soviet ballistic missile submarines and naval bases in a conventional war. Finally, Posen compares offensive and defensive strategies for NATO protection of sea lanes in the Atlantic, arguing that defensive operations could have been successful. In these scenarios, leaders in Moscow might have felt pressures either to initiate the use of nuclear weapons first or, at a minimum, to delegate effective control of nuclear weapons to lower-level military officers, thereby increasing the danger of accidental or unauthorized use of such weapons.

Posen's assessments are certainly plausible, although (as is usually the case in counterfactual analyses) reasonable people can disagree about the likelihood of various contingencies. Future research can therefore further illuminate this issue by examining prenuclear historical cases of inadvertent escalation and war termination. For example, a thorough investigation of the reasons why some conflicts escalated to gas warfare while others did not would at least provided analogical evidence on the likelihood of different nuclear scenarios. Similarly, studies of war termination may provide insights into the conditions under which states facing defeat terminate hostilities (the U.S. government's rationale for the maritime strategy) or make desperate decisions to escalate combat to more dangerous levels (Posen's argument).

In a *very* brief (two page) section of the concluding chapter Posen presents two reasons to suspect that medium and small nuclear powers in the future face even more serious dangers of inadvertent escalation in any conventional war: (1) "they are unlikely to be able to afford the numbers, basing modes, or early-warning capabilities that helped the superpowers to develop survivable retaliatory forces" and (2) "the conventional forces with which these countries have conducted their competition have all regularly engaged in offensive military operations" (p. 201). I would add a third reason to anticipate problems, namely, that many developing states have lower degrees of civilian control over the military, which would compound the organizational causes of escalation identified by Posen. These are strong reasons to be concerned. Posen does not, however, analyze factors which might *reduce* the vulnerability of nuclear weapons and command-and-control between small nuclear powers, such as the absence of stealthy aircraft or sophisticated antisubmarine warfare capabilities in these countries. Yet although Posen does not adequately address the similarities and differences between his superpower cases and the circumstances likely to exist between future proliferators, this was not his main task. Other scholars will be indebted to Posen for giving them a very valuable set of analytical tools to do this important work.

Stanford University Scott D. Sagan

Winning the Next War: Innovation and the Modern Military. By Stephen Peter Rosen. Ithaca: Cornell University Press, 1991. 275p. $33.00.

In recent decades, research and debate on the determinants of military strategy and doctrine have advanced in two main waves, which we may call the *professionalist* and *institutionalist* approaches. The professionalist approach, defined by Samuel Huntington in the late 1950s, focuses on the status of military officership as a distinct profession with its own specialized knowledge and corporate identity. Motivated by technical love of their craft and by their sense of social responsibility, officers seek to enhance the resources and autonomy of the military in order to maximize its capability to carry out its function of securing the state. Civilian intervention in military decision making is unhelpful, because officers know their craft better than outsiders, and often damaging, because it politicizes and deprofessionalizes the officer corps.

The institutionalist approach, advanced by Barry Posen, Jack Snyder, Stephen Van Evera, and Matthew Evangelista in the mid-1980s, starts from slightly different premises but reaches quite different conclusions. Institutionalists recognize the military as a large, specialized, hierarchical organization whose behavior can be understood using the tools of organization theory. Large organizations, including the military, pursue institutional interests in maximizing their budget, prestige, and independence from outside control and in preserving the organization's self-concept, or "essence." Unless monitored from the outside, strategic and doctrinal choices may sometimes respond more to the military's institutional self-interests than to state requirements for military effectiveness. However, civilian monitoring can be difficult, because the military's near monopoly on relevant expertise makes it difficult for outsiders to criticize service arguments effectively (although the immense U.S. postwar civilian defense establishment has ameliorated this somewhat).

Innovation is often resisted, because it threatens the budget, independence, or essence of services or branches that may be deemphasized in favor of others, forced to coordinate more closely, or even lose their traditional missions. Successful innovation will be more likely when the existing leadership and doctrine has been delegitimated by defeat; when civilians intervene to help innovators overcome resistance; or when the innovation supports, rather than challenges, the organization's autonomy and essence.

Stephen Rosen seeks to revive the professionalist approach, arguing that innovation often originates within the military and is rarely instigated or facilitated by civilians. His study, an expansion of his "Theories of Victory" (*International Security* 13[1988]), provides explanations of what he sees as three distinct classes of military innovations: peacetime, wartime, and technological. First, he argues that peacetime innovation is a function of military communities' vision of the future of war and of their criteria for promoting senior officers. Successful innovation depends on insightful imagination, as well as creation of promotion paths that will allow junior officers to advance their careers while pursuing the innovation. This, in turn, requires the support of senior officers. Civilian intervention in the promotion process would be seen as illegitimate and would factionalize the officer corps (pp. 20–21).

Second, innovation in wartime is far more difficult than in peacetime, because time is short, the lessons of combat are often not obvious, and disruption of ongoing operations is risky. Successful wartime innovation, when it occurs, usually requires new measures of strategic effectiveness. Innovators must rethink the connection between operational missions and strategic goals, since success may require not improved execution of existing missions but, rather, their abandonment in favor of new missions and capabilities (p. 110). This process is carried out primarily inside the military.

Finally, for military-technological innovation, Rosen rejects both the institutionalist and "technology-push" explanations in favor of a modified "demand-pull" approach. Technologies are developed based on intelligence estimates of requirements to counter the enemy's current, and expected future, capability, both in terms of technology and doctrine. Successful innovation depends on managing the inherent uncertainty in intelligence. At the research-and-development level, civilian scientist can contribute; but military management is preferred, because officers better understand the combat applications of technology (p. 227). (As a sidelight, Rosen claims that U.S. military research-and-development has been much more cost-effective than most observers realize [p. 46]; this is a question worth further investigation.)

Rosen makes significant contributions, especially in pointing out the importance of promotion paths and strategic evaluation, but does not overturn the institutionalist argument. Internal innovators do often generate new doctrines (and sometimes new technologies), and judicious use of promotion and other internal bureaucratic levers can build constituencies within the service; but actual implementation in creation of new force types or war plans is often blocked by entrenched interests that the innovation threatens. Before World War I, numerous military reformers in all the major European armies pointed out the need to adjust tactics to a battlefield on which the defense had been strengthened by machine guns and barbed wire; but they could not prevail against institutional interests that required offensive doctrines.

Rosen's three peacetime cases illustrate this. The 1930s renovation of U.S. Marine Corps amphibious doctrine, which fitted perfectly with the corps's essence as the spearhead of overseas land operations and enhanced its resource and autonomy claims, was carried though, in part, by promoting amphibious-minded junior officers and without civilian assistance.

Also in the interwar years, Rear Admiral William Moffett's strategy of coopting senior nonaviators to support promotion of junior aviators made all the difference between the advance of American aircraft carrier doctrine and the stagnation of the British Fleet Air Arm at the same time. However, Rosen is wrong to play down the importance of the Pearl Harbor attack. While an effective combat doctrine was developed before the war, it did not replace the Navy's battleship-centered war plan until Pearl Harbor eliminated the battleships.

Finally, in the 1950s, airmobility doctrine began to make progress within the army when pilots were no longer looked down on as glorified taxi drivers and were, instead, considered eligible for promotion to combat commands. However, sizable airmobile combat units were not formed until Defense Secretary Robert McNamara demanded their creation in 1962. Rosen minimizes the importance of this intervention by suggesting that McNamara may have been primed by aviation advocates within the army, but the crucial point is that they could not prevail without outside assistance.

In one sense, Rosen's promotion path argument is worth more weight than he gives it. One of the most important motivations for military resistance to innovations is that they create requirements for new professional skills and devalue the skills of officers trained in the old doctrine, damaging their prospects for promotion.

So how do we win the next war? Rosen suggests that in an environment in which we cannot predict the likely scenarios that we may face or the missions that will be required and where limited budgets will not permit large-scale acquisition of new weapons or creation of new force types, we must rely instead on flexibility and innovation. In particular, he recommends relying on analysis and simulation of as many different contingencies as possible and researching a variety of technologies, rather than investing heavily in production of any one.

This policy advice is eminently sensible (and rapidly becoming conventional wisdom), although it is not really dependent on the rest of the book. More interesting would have been recommendations directly tied to the author's theoretical arguments. If successful peacetime innovation requires new promotion paths, can we design ways to institutionalize opportunities for younger officers to make their careers pursuing innovation doctrines or technologies? If wartime innovation depends on improved measures of strategic effectiveness, can we improve the odds that focused and timely analysis will be conducted, disseminated, and acted upon in future conflicts? Attention to these questions might help us design useful reforms.

University of Michigan CHAIM KAUFMANN

History and Strategy. By Marc Trachtenberg. Princeton: Princeton University Press, 1991. 292p. $44.50 cloth, $14.95 paper.

History and Strategy is a collection of seven essays by Marc Trachtenberg, all but one of which have been published elsewhere. Their subjects are diverse: the early evolution of American strategic thought, the origins of World War I, American nuclear strategy in the early 1950s, the nuclearization of NATO, the Berlin crisis, the influence of nuclear weapons in the Cuban Missile Crisis, and the relevance of historical analysis. Many of the essays are excellent, all provocative; but they do not cohere as a book.

The chapter on American strategic thought sets out to answer a puzzle, Why did the "golden age" of strategic analysis "hit something of a dead end in the 1960s"? It never really provides an answer but develops an insightful critique of the literature. Trachtenberg faults these strategy analysts, especially Thomas Schelling, for narrowly conceptualizing war, peace, and strategy as problems of crisis management and bargaining. By focusing almost exclusively on the manipulation of risk and "the purely military side of war causation," American strategists reduced strategy to tactics and analyzed these tactics outside of any political and historical context. To reinvigorate the study of strategy, it is necessary to ground deductive, abstract analysis in a thorough understanding of the processes that have led to the development of nuclear arsenals and the conflicts to which they are relevant.

The second essay in the book is the author's controversial attack on the thesis that war broke out in 1914 because of the rigidity of military plans and the loss of control they engendered. The gist of his argument is that the Schlieffen Plan was based on the accurate political judgment that France would not remain neutral in a Russo–German war; that German political leaders understood and approved the Schlieffen Plan; and that they, not the military, made the decision for war. In Russia, too, Trachtenberg maintains, political authorities understood the meaning and consequences of mobilization.

From World War I, the book shifts back to nuclear strategy and how Americans adjusted to the loss of their monopoly on nuclear weapons. The author argues that anticipated shifts in the military balance played a critical role in shaping grand strategy and specific policies, especially with regard to Korea. He documents how seriously preemption and other aggressive strategies were taken at the highest levels of the government in the early 1950s. No final decision was taken against preemption until 1954. The chapter as a whole illustrates the myopia of the Pentagon and the Eisenhower administration. For both, the military balance was the only reference necessary to explain the twists and turns of Soviet foreign policy.

A follow-on chapter examines U.S.–West European military relations during the 1950s. Trachtenberg challenges the conventional wisdom that the nuclearization of NATO was an attempt to bridge the gap between European defense requirements and the political and economic constraints that ruled out large conventional forces. He insists that a nuclear-oriented strategy made good military sense in the early 1950s. The real debate in Washington was between the army's conception of a "forward defense" and the air force's preference for a "peripheral strategy" that contemplated withdrawing from the continent. By 1953, technical breakthroughs and the growth of the nuclear stockpile made a nuclearized forward defense feasible. Such a strategy was preferable to the Eisenhower administration and the Europeans for strategic and political reasons.

The next two chapters look at two of the tensest confrontations of the Cold War: Berlin in 1959–62 and Cuba in 1962. Trachtenberg makes a compelling case that the Soviet challenge to the Western position in Berlin was motivated primarily by Soviet fears that West Germany would soon acquire nuclear weapons. From the Soviet perspective, Konrad Adenauer and the United States were intent on changing the status quo, Moscow, on maintaining it. In the end, the Unites States came to accept the division of Germany and even to view it as a positive development. The crisis also indicated just how difficult nuclear weapons made it for either side to threaten war credibly.

The Cuba chapter is now out of date because of the new information that has since become available, especially on the Soviet side. The question it poses is still timely: What role did nuclear weapons play in shaping American and Soviet policies in the crisis? Trachtenberg argues that the lopsided strategic balance led to asymmetrical nuclear anxieties and (here I strongly disagree) to a "balance of resolve" that favored the United States and was reflected in the outcome of the crisis. He is on firmer ground in arguing that President Kennedy and Secretary of Defense McNamara were constrained by their fear of loss of control from exploiting their strategic advantage. Soviet leaders, who did not see this, exaggerated the threat of an American nuclear attack.

The final chapter, "Making Sense of the Nuclear Age," argues that historical scholarship can facilitate a reconceptualization of the fundamental problems of foreign and military policy in the nuclear age. This essay should have introduced the volume and gone on to identify the themes that would be examined in the subsequent chapters. With a sharp introduction, the individual chapters could have been more focused and the interesting but irrelevant essay on World War I dropped from the collection. The conclusions should have offered the author's reconceptualization of nuclear strategy, thereby validating his initial claim and enhancing the volume's value and interest to scholar and policymaker alike.

University of Pittsburgh Richard Ned Lebow

International Theory: The Three Traditions. By Martin Wight. New York: Holmes & Meier, 1992. 286p. $39.95

Martin Wight can be considered the founder of the English school of international relations theory, which emphasizes the cultural and moral presuppostions of international relations, its historical dimensions, and the works of great thinkers in the Western tradition. This book consists of Wight's lectures at the London School of Economics during the 1950s and 1960s, published 20 years after the author's death. In a brilliant introduction, the late Hedley Bull, Wight's most distinguished student, argues that Wight's approach "provides an antidote to the narrow and introverted character of the professional academic debate about International Relations, . . . opening it out to wider intellectual horizons" (p. xix). So it does: indeed, much of the merit of this work lies in its claim that contemporary controversies are extensions of old disputes, carried out in previous centuries by thinkers more deeply imbued with philosophical subtlety and historical understanding than we are. Such an argument allows us to fit ourselves better into a collective tradition, giving us perspective and, one hopes, humility. Publication of *International Theory* is therefore an important contribution to the literature, which should be read by American students and scholars.

International Theory is built around Wight's threefold classification of thought on international relations into Realism (sometimes labelled Machiavellian), and Revolutionism (stylized as Kantian), and rationalism (viewed as Grotian). Realism denies the reality of international society, sees states' interests as essentially in conflict, and justifies action by necessity or by success. War is inevitable; history is cyclical. Revolutionism is an odd, hybrid category, meant to encompass Kant, Cobden, Lenin, Hitler, and Stalin. It seems to me that generalizations about revolutionists fit the latter better than the former. Kant, indeed, stressed that world peace would be promoted by republican constitutions within states. But he was no utopian about human nature; he sought a loose federation, rather than a world state; and he did not believe in an authoritarian transformation of the world. Wight himself suggests in his conclusion that "soft" revolutionists, from Kant to Nehru, should be distinguished from "hard" revolutionists, such as the Jacobins and Marxists (p. 267). Rationalists—toward whom Wight seems to lean—regard interstate society as real, compromise among interests as usually possible, and moral dilemmas as soluble through a combination of respect for rules and concern about consequences. War is to be avoided when possible and waged as humanely as possible when it occurs.

Wight admits that there is a "confluence" of these traditions; he is aware of the perils of classification. In his introductory essay, Bull argues that "the account of the three traditions will not bear all the weight that Martin Wight sought to place upon it" (p. xviii); and I agree. The hybrid character of his revolutionist category muddies his analysis of revolutionism. However, the categories of realism and rationalism facilitate some of Wight's most striking insights: such as "the Hobbesian character of the United Nations," never more evident than in the war against Iraq (p. 34); realism's expansionary conception of national interest versus rationalism's contraction of interests (p. 126); and realism's ambivalence about our ability to ascertain others' interests versus rationalism's claim for the objectivity of distance (pp. 146–47). Wight's categories work better for interests, international society, and the ethical basis of action than they do for power, balance of power, bargaining, and war. World politics is multidimensional, and views on bargaining or the balance of power are not cogently defined by views on international society or ethics. In spite of Wright, rationalists need not believe in the inviolability of promises, especially if such promises seem to conflict with peaceful change and adaptation to new situations.

Wight is definitely a thinker in the Western tradition, making few references to other cultures' views of international relations. Consistent with his Christian piety, one might fear that he would therefore adopt an uncritical view of the actions of the west toward non-Western peoples. Nothing could be further from the truth. His brilliant chapter 4 on relations with "barbarians" extensively documents Western justifications of savage cruelty and wars of extermination toward these peoples. Indeed, he identifies "extermination to eradicate heresy, to purify society, for scientific social engineering, for the sake of the cause," as "specifically Western." Islam, he says, "may have invented the holy war, but it did not exterminate" (p. 229). For critical insights such as these, from someone *within* the Western tradition, *International Theory* is worth serious reading and reflection.

Wight's greatest oversight, which Bull also mentions, is his neglect of the scientific or behavioral search for laws of action (or contingent generalizations) about world politics. Wight does not defend his view that cumulative knowledge is impossible in world politics. He claims that rationalists do not see international politics in terms of laws of behavior but that most contemporary behavioralists, who seek laws, are also rationalists. Yet American international relations specialists, even the most behavioral and rationalist, should read this book in order to place themselves in the tradition of thought in international relations, appreciate the insights that come from studying particularities, and see how history and philosophy can enrich our study by providing us with insights and even, one dares to say, testable hypotheses.

Harvard University ROBERT O. KEOHANE

The Tet Offensive: Intelligence Failure in War. By James J. Wirtz. Ithaca: Cornell University Press, 1991. 290p. $34.95

Strategic military surprise has been a satisfying genre in security studies. Investigation of a complex puzzle—why states and leaders seem so vulnerable to catastrophic surprise when the resources devoted to avoiding them are substantial, the stakes huge, and their suspicions about antagonistic states very high—has yielded a relatively simple and elegant theoretical explanation. Even better, it seems to hold across cultures, times, and personalities, with later studies providing additional confirmation.

James Wirtz's volume is a case in point, very much in keeping with other case studies of strategic surprise. Thus, it has little in terms of innovation on this subject. However, it is more than satisfactory in recounting the events of the Tet Offensive and is clear and precise in the application and refinement of the available theory. The theory and contributors to its development are insufficiently cited. Readers unfamiliar with the subject may not realize how firmly the book is set within an existing theoretical perspective.

Is vulnerability to strategic surprise due to the attacker's *deception* or the victim's perceptual mistakes? Is defender misperception due mainly to the combination of attacker secrecy, the ambiguity of available data, and the distractions of other information ("noise") or mainly to distortions imposed by defender preconceptions, beliefs, and expectations? Are the key mistakes made by top officials or by organizations and lower-level personnel? Finally, do the misperceptions spring from standard cognitive processes or "motivated biases"? Wirtz's analysis puts considerable emphasis on deception (more precisely, on the interplay between the victims' perceptual predilections and the enemy's elaborate deception effort). Like analysts of other cases, however, he finds that the victims' preconceptions in this instance were crucial. There was enough information available, clear enough in import, for the Americans to have anticipated the Tet Offensive. Their preconceptions (bolstered by the deception) got in the way. He parallels other studies in locating the major mistakes mainly at the top, with leaders and central headquarters analysts. Finally, most of the distorted analysis they displayed appears to have flowed from unmotivated bias. Along the way, he spots instances of all the standard cognitive errors, such as: (1) assuming more action–reaction linkages with the enemy than actually exist; (2) letting preoccupation with one's own strategy override efforts to grasp the enemy's;

(3) crying wolf; and (4) comfortably explaining away discrepant information.

As the emphasis on deception is the most innovative feature of all this, it is unfortunate that it is open to challenge. The massing of enemy forces on the borders, especially near Khe Sanh, is depicted as a deception about the location of the forthcoming attack designed to keep American forces away from the cities. Thus, a Vietcong attack there would face only the (ARVN) and could more readily (it was hoped) provoke a popular uprising. Maybe so, but he admits that the evidence that deception was the purpose is incomplete; and the evidence there is fits the equally plausible explanation that the North Vietnamese wanted to exploit the Americans' presence at the border to inflict heavy casualties and maybe another Dien Bien Phu, while *simultaneously* hitting the ARVN and the government in the cities. This could make Khe Sanh less a deception than part of a two-pronged attack, which would explain why the enemy did not just feign an attack but went into it vigorously, taking heavy casualties. Hanoi may have been seeking the same damaging political–psychological results via *either* of the two attacks. Whether one was simply intended as a deception cannot be definitively determined, and the conventional understanding of the role of deception in achieving a strategic surprise is left unchanged. His analysis is a stimulating view that will have to await further information from Hanoi to be fully confirmed.

Even the claim that Westmoreland overreacted (was deceived) in reinforcing Khe Sanh is problematic. After all, as the author notes, another Dien Bien Phu would have had an immensely damaging impact on the war effort, and Westmoreland was under great pressure in this regard because of McNamara's huge investment in the idea of a defensive barrier across the northern border and President Johnson's intense preoccupation with that area. He would have had to keep very large forces there in any case, deception or not.

For our general reflections on the Vietnam War, the book is valuable in many ways. For instance, Wirtz finds little evidence of willful distortion of information. Most of that which challenged official perceptions (alternative analyses, dissenting intelligence findings, discrepant information) was passed along to very high officials and given at least some consideration. The officials do not, in his account, appear to have aggressively evaded confrontation with criticism and dissent.

He disputes the view that headquarters in Saigon deliberately understated the size of Vietcong forces—the position of the CIA's Samuel Adams at the time and since. Military Assistance Command Vietnam's estimates were largely correct, he says; but Hanoi, accepting the veracity of the same reports Adams used, arrived at the same inflated estimates of their support in the south that he did. American leaders knew that a major Vietcong offensive would be militarily foolish in view of actual Vietcong strength. Hanoi overestimated its strength in deciding to mount what the United States, therefore, considered an improbable kind of attack. This is a nice touch in the author's assessment, although finding that attacker misperceptions played an important role is not original with Wirtz and this case.

He also concludes that those suspicious of the periodic enemy-offered truces (because the enemy exploited them) or of Hanoi's offers to negotiate were, on the whole, correct. The North used such offers to help set up the Tet Offensive; and taking advantage of the Tet ceasefire was only exploitation beyond the norm.

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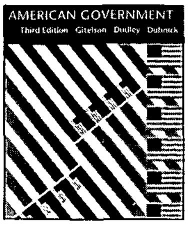

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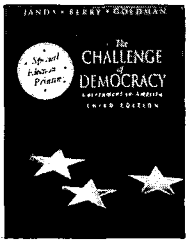

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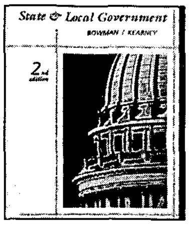

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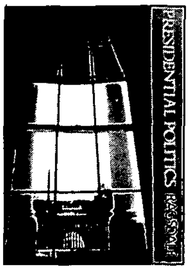

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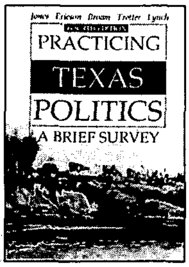

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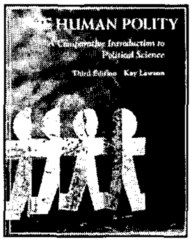

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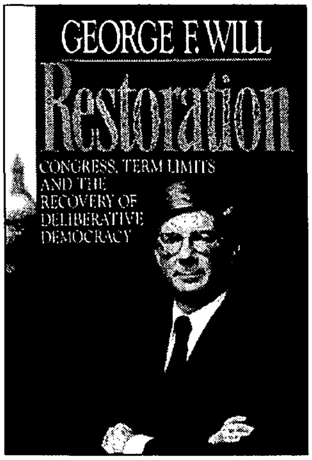
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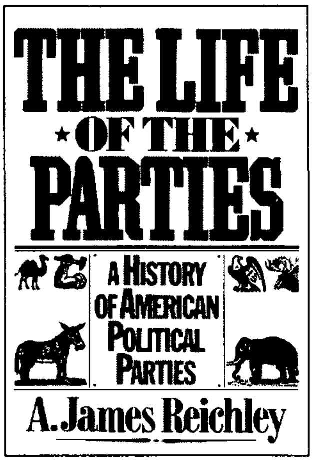

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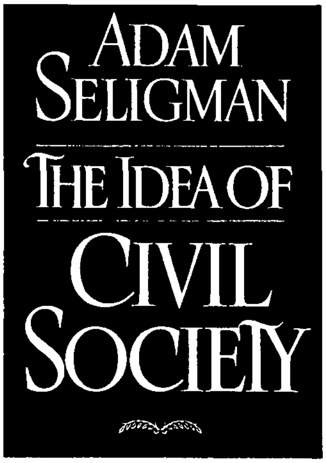

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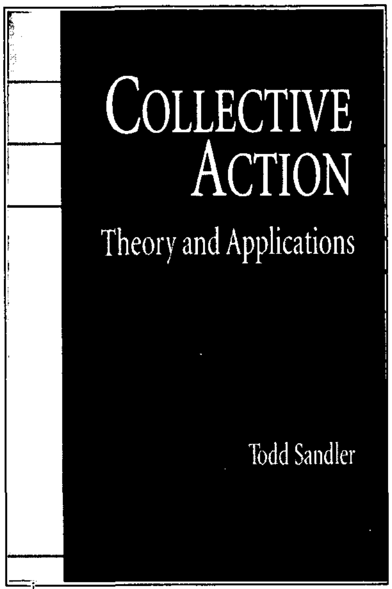

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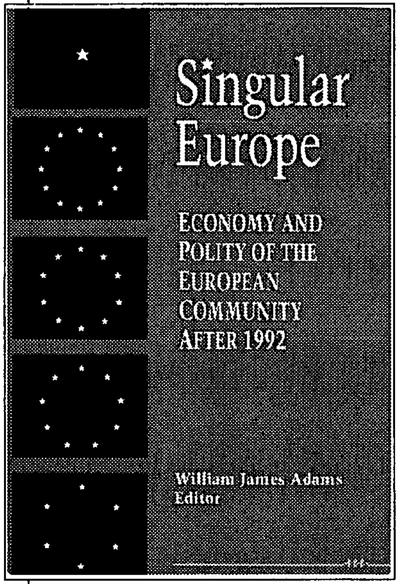

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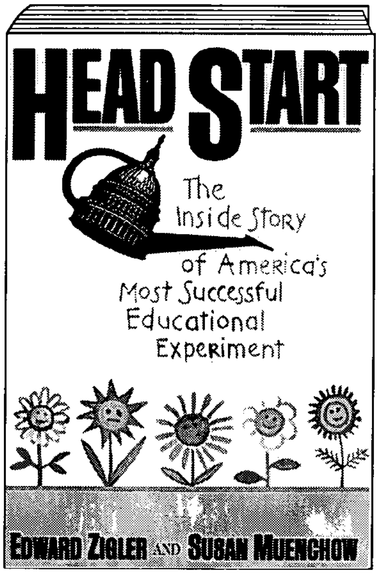
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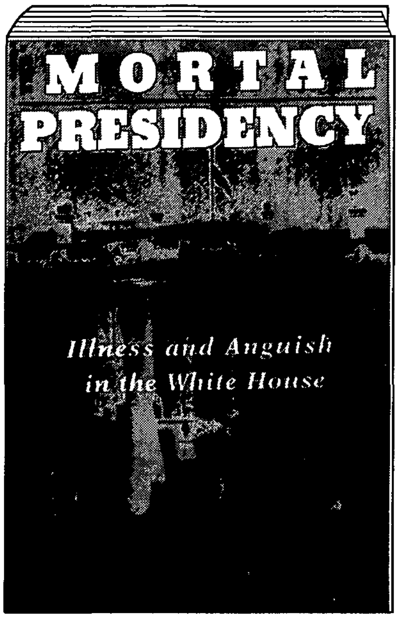
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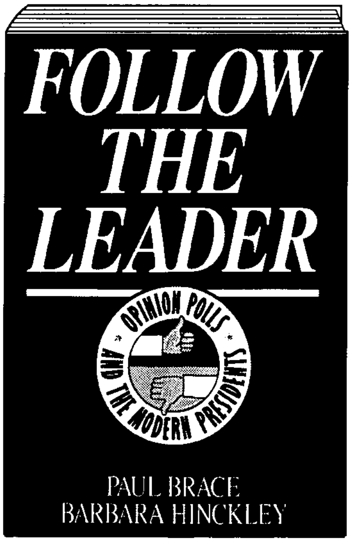
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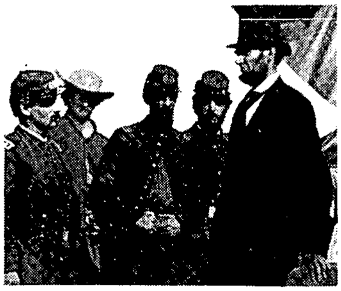

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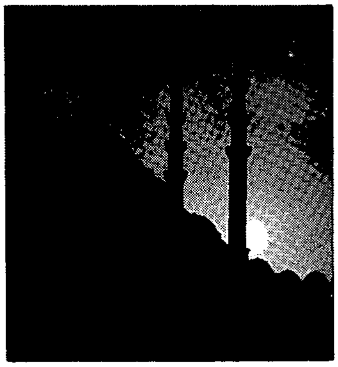

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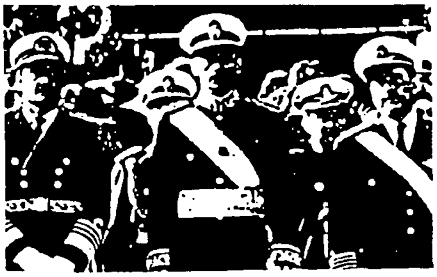

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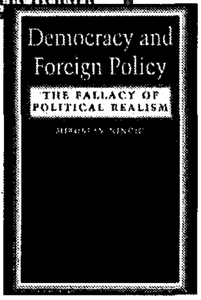
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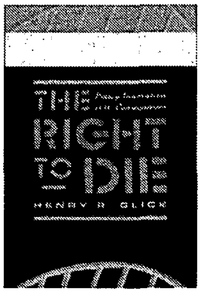
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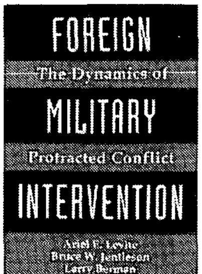
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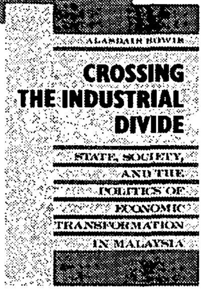
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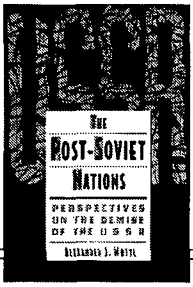
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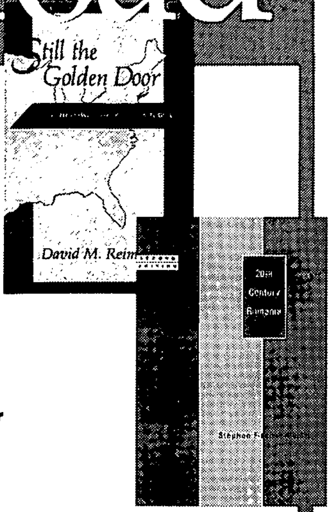

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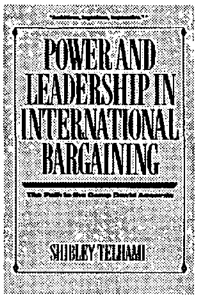

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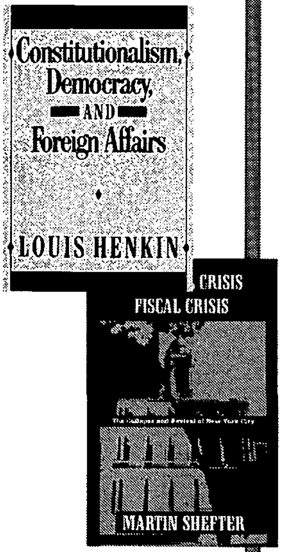

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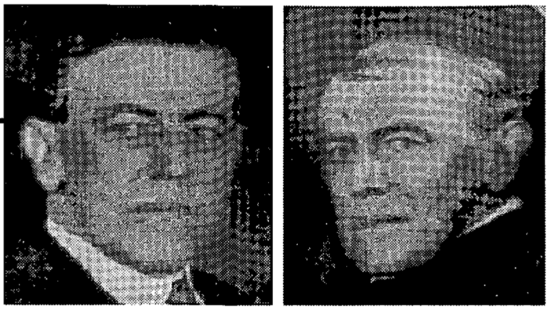

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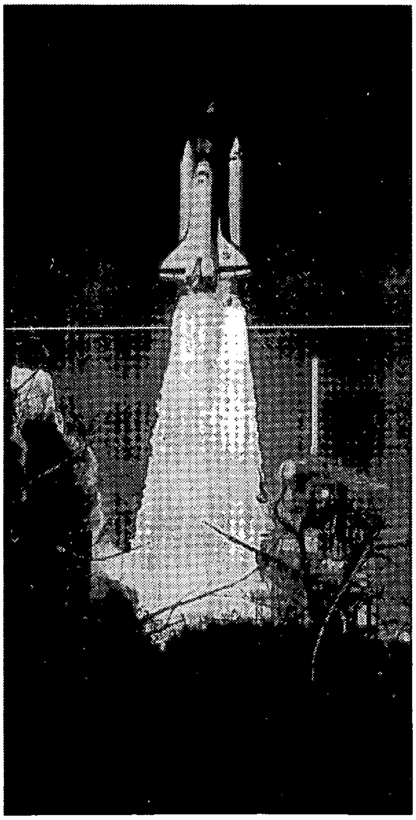

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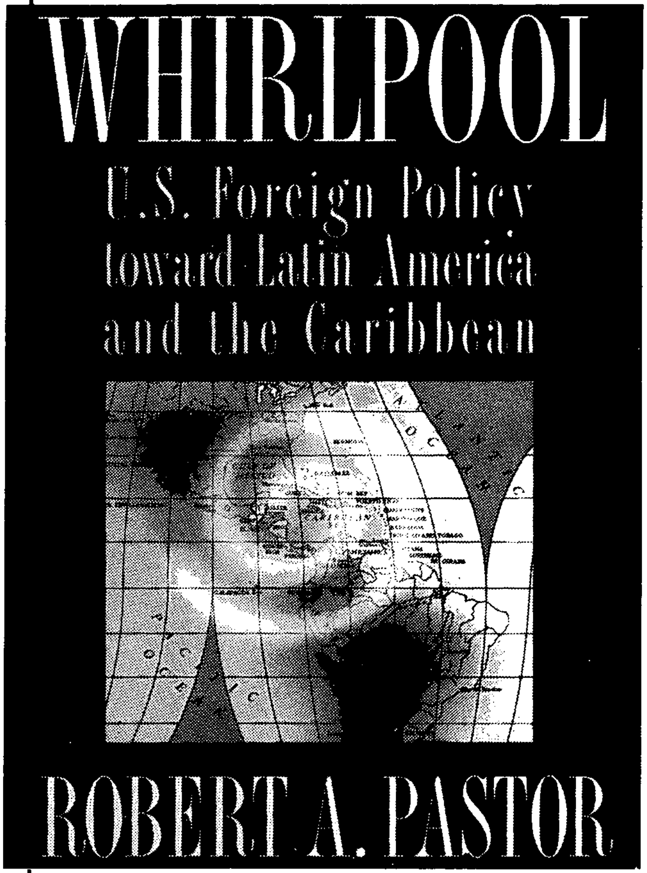

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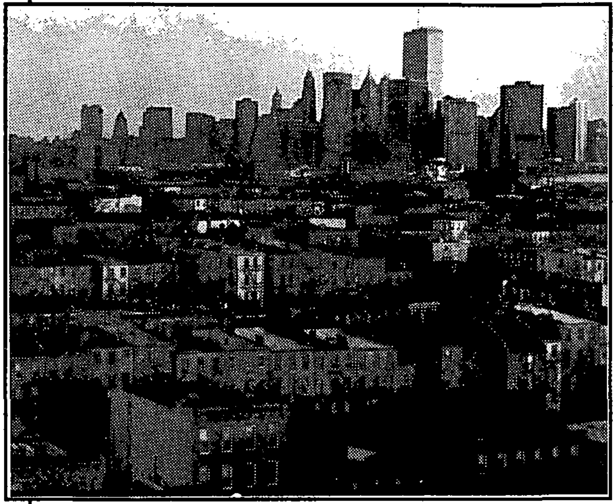

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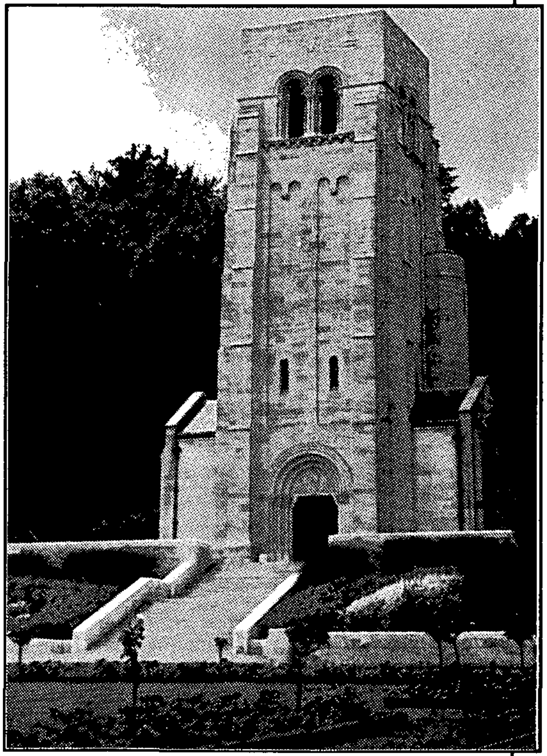

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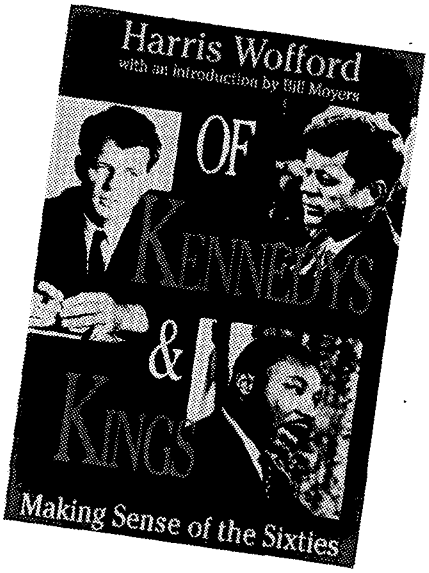

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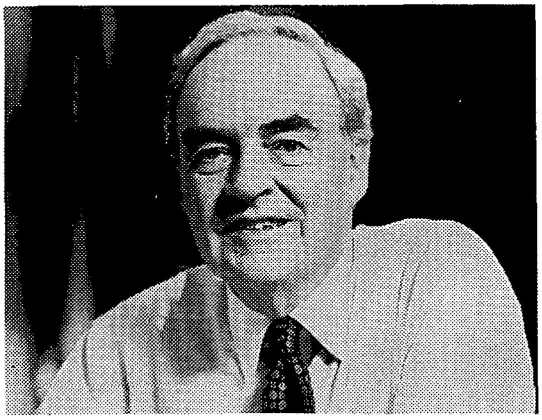

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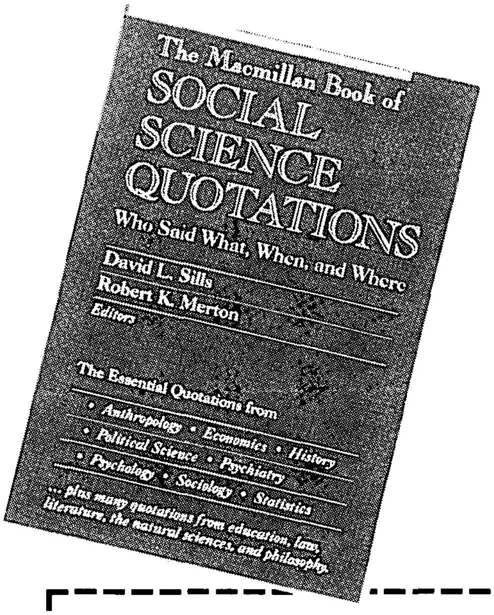
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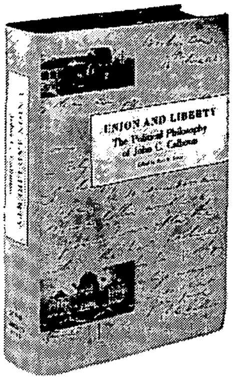

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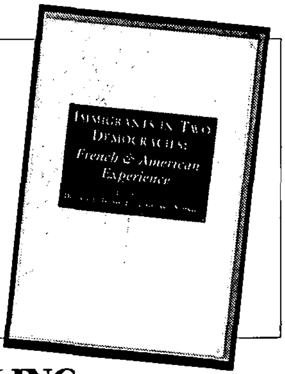

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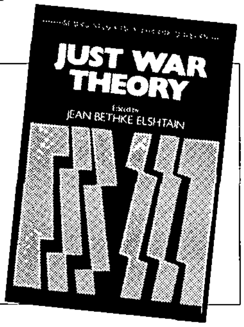

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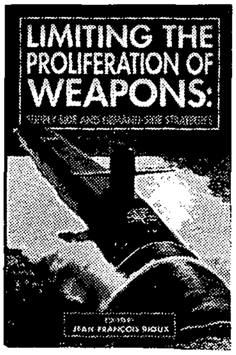

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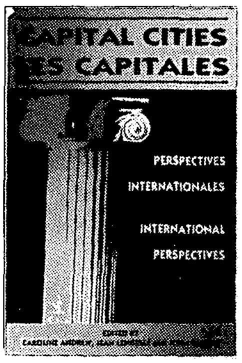

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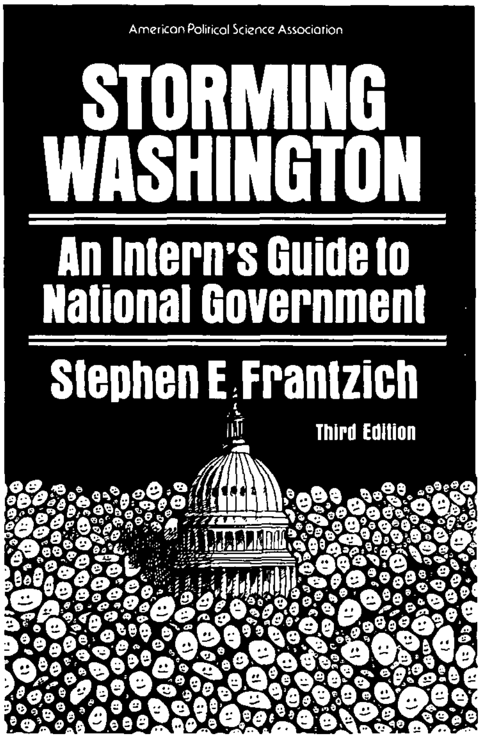

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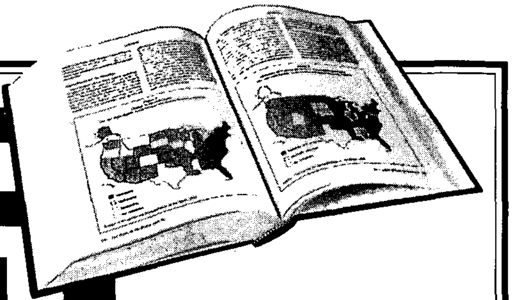

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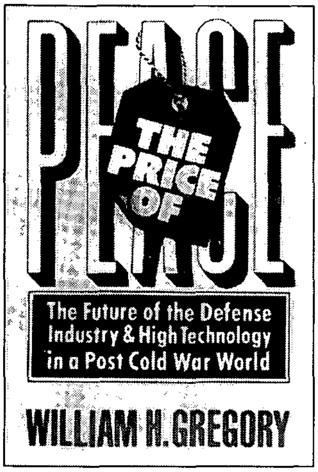
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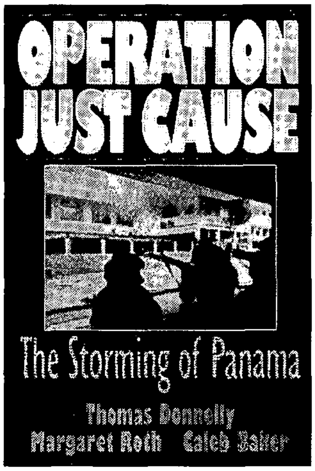

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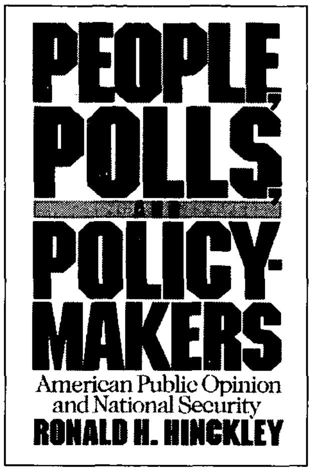
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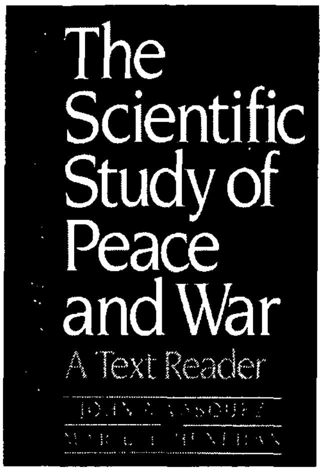

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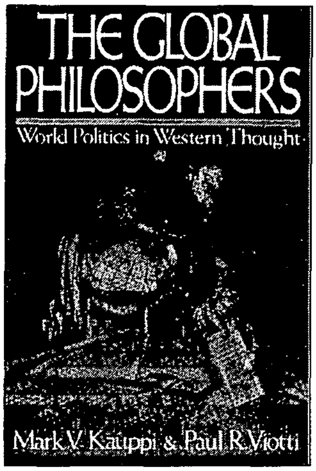
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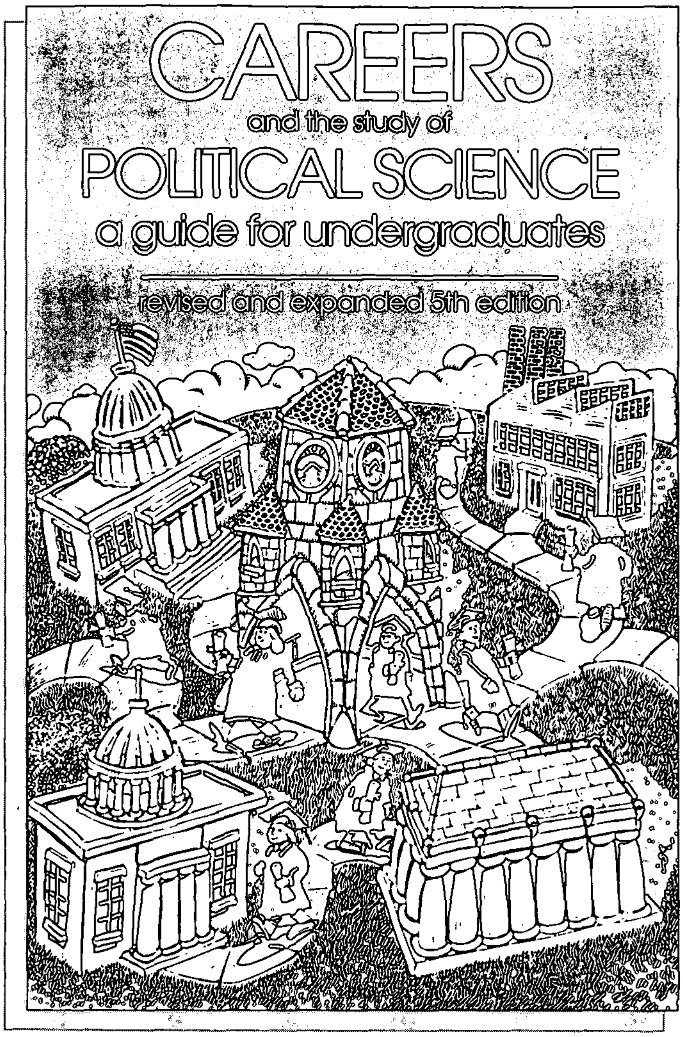

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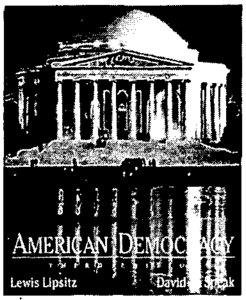

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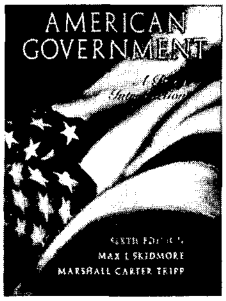

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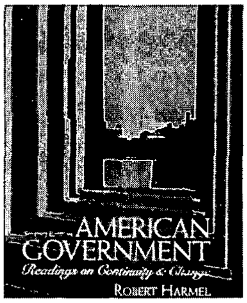

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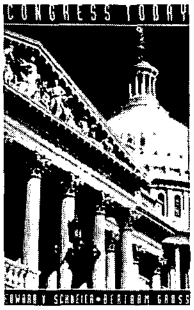

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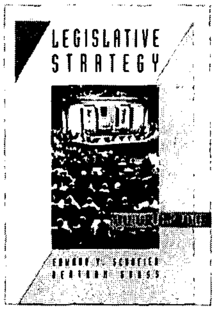

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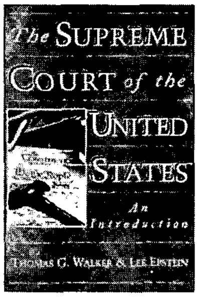

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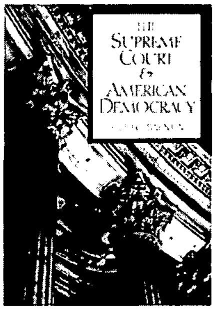
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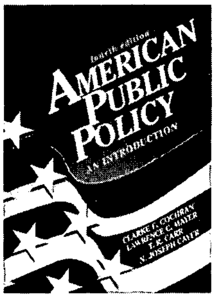
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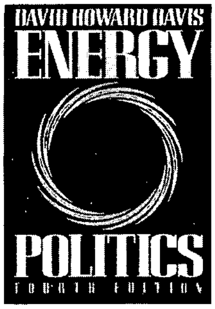
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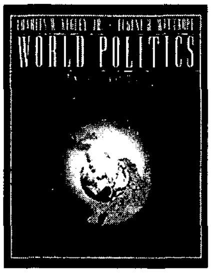

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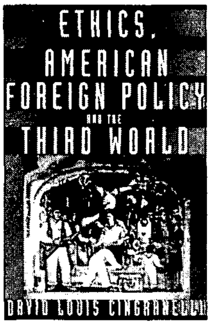

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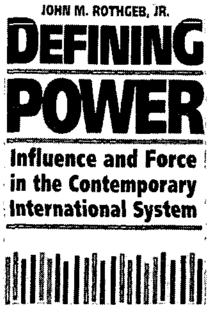

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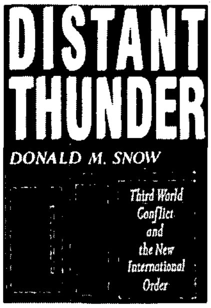

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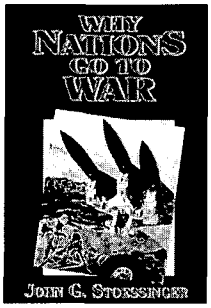

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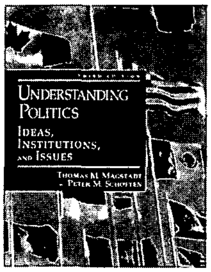

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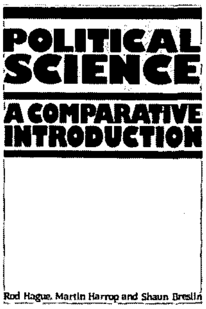
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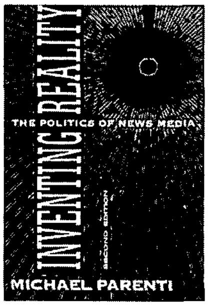
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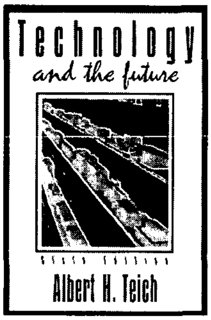
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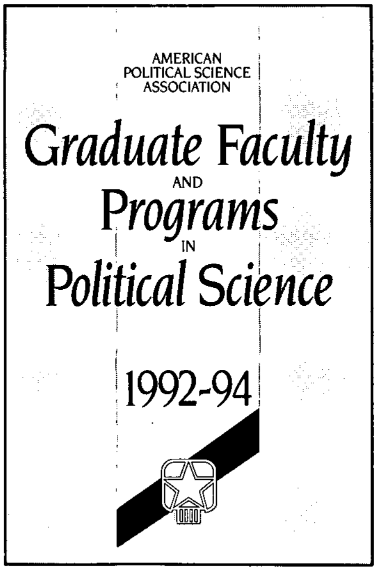
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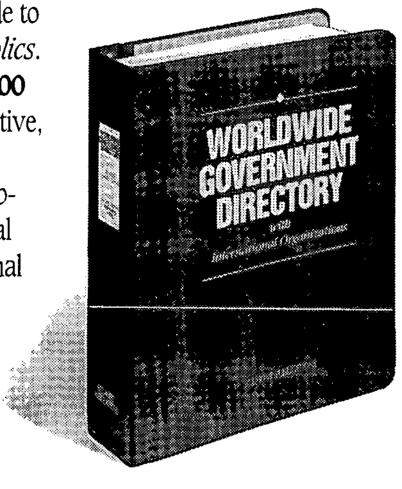
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